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THE

MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

An Illustrated Journal of Practical Microscopy.

EDITED BY

GEORGE E. DAVIS,

F.R.M.S., F.C.S., F.I.C.,

ETC., ETC.

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PREFACE.

AFTER a period of four years' existence, it is with many regrets that we have to suspend the publication of THE MICROSCOPICAL NEWS. During the past seven or eight months the Editor's time has been so much occupied with matters other than microscopical, that he has felt for some time it would be better to suspend publication until he had more time at his disposal. Although the Journal has not been pecuniarily successful in the past, we believe it had arrived at a stage at which the revenue would have balanced the expenditure, and this is all that could be desired in a Journal interesting so small a proportion of the community as microscopists must be. We have gained some little experience of the world by means of this publication, and it has many times led us into pleasant paths and amongst pleasant company, so that it is on this account more particularly that we regret severing our connection with our readers. When first the Northern Microscopist was announced, we were asked in the great metropolis who was going to be foolish enough to perpetrate such a deed. We must say we have never for one moment regretted the publication of our first and succeeding numbers, and we hope that the four volumes may be found to contain matters interesting to the experienced microscopist and of use to the beginner.

Did space allow, we would fain say a few words regarding Microscopical Societies and their management, their aims and objects also, as we really came into existence with a view of recording their proceedings, but any superfluous microscopical literature can always find an outlet in any of the ordinary channels.

Our readers have been presented with the whole of Professor

Abbe's papers on the microscope, and this subject will provide sufficient food for reflection for some time to come.

The Editor desires to thank those microscopists who have aided him during the present year, and in fact in previous years also, but more particularly has the help been valuable during the preparation of the present volume, when it was hardly possible to give sufficient time even for the reading of proofs.

Our Verification department will still continue. We shall be glad to verify any objectives that may be sent us, and will feel grateful also for notes upon microscopical matters, useful for future issues of the Editor's work on Practical Microscopy, an entirely new edition of which is now preparing.

With this, we conclude, and wish our readers

"THE COMPLIMENTS OF THE SEASON."

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No. 37. *JANUARY.* 1884.

PRACTICAL PROCESSES IN VEGETABLE HISTOLOGY.

By L. Oliver, in Rev. Sci. Nat., 1882.

Taken from the Journal of the Royal Microscopical Society.

(Continued from page 322, Vol. III.)

II. Fixation of Forms.

THE ternary parts of the plants being generally tolerably rigid, it is only necessary to fix the proteid matter (protoplasm, nuclei, vibratile, cilia, &c.). The following agents are employed for this purpose.

Absolute alcohol. When absolute, alcohol fixes the protoplasm without contracting it. It can be made to act directly on the preparation to be examined, or upon the organs before making sections. Strasburger has studied in the latter mode the formation of the cells in *Iris pumila.* By immersing *Spirogyra orthospira* in absolute alcohol at different hours of the night he succeeded in fixing the different phases of the division of the nucleus in this alga, which it then became very easy to study by daylight (without its changing) the day after and the following days. The same observer succeeded in retarding division until the morning by placing the *Spirogyra* in a room without heat in November. He was thus able to follow under the Microscope all the phenomena of the division, and to fix them at the most suitable moment by immersing the plant in absolute alcohol.

Chromic acid.—L. Guignard has successfully employed chromic acid to fix the nuclei in the embryo-sac in the Mimosæ.* The good results he obtained with it mark this reagent as one of the most valuable in vegetable microchemistry.

Osmic acid.—Osmic acid, whilst fixing the form, has the advant-

* Bull. Soc. Bot., 25th June, 1880.

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age of giving transparency to the protoplasm and the cell-walls, but has also the inconvenience of destroying the protoplasm after some hours. Strasburger has nevertheless used it in his observations on the division of nuclei. He placed the plants in water containing 1-500th of sugar, and added one or two drops of a 1 per cent. solution of osmic acid.

Vignal* and Certes† have called the attention of naturalists to the good results obtained with osmic acid for fixing instantaneously the forms of the lower organisms (*Noctilucæ*, infusoria, algæ, zoospores, microbes of virulent diseases, &c.) Generally it is sufficient to expose the organisms on the slide for five minutes to the vapours of a 1 per cent. solution of osmic acid. But if they are very contractile it is preferable to treat them directly with the liquid acid after all disturbance of the slide has ceased.

Certes‡ has succeeded in doing away with the corrosive action of osmic acid. He places the organisms to be examined in a test-tube containing 30 c.cm. of distilled water or a few drops of the water of which he intends to make a microscopical analysis. He adds to it 1 c.cm. of half per cent. osmic acid. In a few minutes he fills up the test-tube with water, and allows it to rest for twenty-four or even forty-eight hours. All the algæ, spores, bacteria, monads, vibriones, amœbæ, and infusoria which originally swarm in the water are then deposited at the bottom of the test-tube. They are collected by means of a pipette, after the greater portion of the liquid has been decanted.

For eleven months we have preserved, in the same test-tube in which they were killed, some specimens of *Monas* which, during life, were very active. Their form has hitherto undergone no alteration. It is exactly the same as at the moment when they were attacked by the osmic acid.

Taking our stand on the fixative properties of this agent, we have attempted to make use of it to determine the parts of an organism endowed with spontaneous motility. We had to decide whether the long caudal filaments, the existence of which we had recognized in the *Bacterium rubescens* of Ray Lankester, are contractile, and whether they are active or passive in locomotion.

With this object we poured into two watch-glasses some distilled water, and a few drops of the water in which they were multiplying abundantly. We added to the contents of one of the two watch-

* "Recherches histologiques et physiologiques sur les Noctiluques," Arch de Physiol., 1878.

† "Sur une méthode de conservation des infusoires." Comptes Rendus, 3rd March, 1879.

‡ "Sur l'analyse micrographique des eaux," Comptes Rendus, 14th June, 1880.

glasses a drop of osmic acid properly diluted, and then added to it distilled water.

When, after a rest of twenty-four hours, we coloured the organisms in the latter glass by means of reagents, of which we shall speak later, we succeeded in showing the long filaments. This was, on the contrary, impossible with the organisms in the other glass; a phenomenon which we attribute to a contraction of the filament in the latter case, and to an absence of contraction in the case of fixation by osmic acid.*

Alcoholic solution of corrosive sublimate.—The effect of this solution employed as a fixative is rapid, but of very short duration. It is used with advantage in studying aleurone.

III. Contraction.

It is known that protoplasm, either free like the plasmodia of the Myxomycetes, or surrounded by a ternary membrane, as in multicellular plants, has at its periphery a hyaline layer, which remains in perfect continuity with the rest of the protoplasm, though distinguished from it by its hyaline appearance, and a greater refrangibility. In the interior of the protoplasm a border of the same nature surrounds the vacuoles when there are any. It is this membranous layer which regulates the osmotic phenomena of the cell. It is very permeable to water, but very little so to the salts which are dissolved in it, so that on placing the cell in pure water or in water charged with salts, the capacity of the vacuoles is increased or diminished, the protoplasm is dilated or contracted.

Amongst the substances which produce the latter effect must be mentioned solution of sugar, weak aqueous solution of chlorate of potash, *dilute* alcohol, glycerin, and sulphuric acid. These agents contract the protoplasm to the extent of detaching it from the cell-membrane. At the same time they give it a consistency which enables it to be better distinguished.

Solution of sugar, introduced gradually into the preparations, contracts the vacuoles without killing the protoplasm; when the cell-sap is abundant, as in old cells of *Spirogyra* and *Œdogonium*, it may happen that the volume of the protoplasm will be reduced one-half.†

Alcohol always kills the protoplasm. It contracts it only when dilute, the slower its action the more marked is its effect. Contracted by this agent, the protoplasmic substance becomes hard and resisting.

Glycerin produces an analogous result, with this difference however, that the protoplasm does not become so rigid.

* Bull. Soc. Bot., iii. 22nd July, 1881. See this Journal, ii. (1882) p. 640.

† P. Van Tieghem, 'Traité de Botanique,' p. 473. Paris, 1882,

Sulphuric acid acts in the same way, with more energy and rapidity. It is important therefore to suspend the action as soon as the contraction has taken place. It would destroy the protoplasm if the action were prolonged.

Mineral acids generally behave in a similar way.

These different substances, frequently employed in the examination of the protoplasm of the higher plants, can also be applied to the study of the lower cryptogams which the simplicity of their structure places at the confines of the two organic kingdoms. Dilute alcohol, glycerin, and the mineral acids, by absorbing water, reduce the bulk of the protoplasmic masses not surrounded by cell-walls, and destitute of vacuoles. We have used them successfully to determine the general construction of the body of *Monas Okenii* Ehr., and to show by that that this microbe, absolutely destitute of ternary envelope, must be removed from the bacteria and associated with the nudo-flagellate organisms.

Knowing the means of rendering the tissues transparent, of contracting the organisms, and of fixing them in their forms, we must now consider what kind of histological elements or products of the vegetable economy are capable of being revealed by means of crystallization, destruction, or colouring. In each of these three cases we shall follow the inverse order to that which we have hitherto adopted; instead of indicating, for each reagent, the different substances for the determination of which it is appropriate, we shall examine the different substances, and for each one point out the microchemical operations which belong to it.

(To be continued.)

FUNGUS FORAYS IN 1883.

THE Cryptogamic Society of Scotland held its annual meeting this year at Dumfries, but with what success we have had no report. The time was rather early for Fungi, and consequently they were not numerous. *Lactarius capsicum*, Schulz, was one of the most noteworthy of the "finds."

The Essex Field Club met in Epping Forest for their annual Foray on the 29th of September, but the day proved anything but a pleasant one, and consequently, as the foray was to be confined to less than three hours, the results were not extraordinary. During the greater part of the afternoon a persistent downpour prevented outdoor investigations.

The Woolhope Field Club opened its week of meetings by the arrival of guests on the evening of October 1st, and excursions

were planned for the four succeeding days. A detailed account was published in the *Gardeners' Chronicle* for October 13th. The general impression was certainly that in the neighbourhood of Hereford, as well as in some other localities in Britain, the present was by no means a prolific year for fungi. From the North it has been reported that at one time the quantity observed was quite equal, if not above the average. In the New Forest this also seemed to be the case; whilst in Cornwall the number was declared to be less than had been known for many years, and in many parts of the Eastern counties the same complaints were made. In Warwickshire, as far as we could judge, the number both of species and individuals was unusually small. In Epping Forest we have seen during the past Autumn fewer fungi than we have observed for many years. Hence we infer that though the general character of the year in England was unfavourable to the production of a good crop of fungi, there were in a few localities as many as are usually to be found in an excellent year. Fungi would seem to obey no law, or if they do, some law which is at present inexplicable to us, since in one place they have been scarce, and in another plentiful, at the same time.

The character of the Fungus flora around Hereford seemed to be rather marked by the occurrence of numerous species of *Cortinarius*, and at the same time Epping Forest, which usually furnishes several species in considerable numbers, yielded this year only here and there a solitary specimen.

Of the specimens found around Hereford, the following is an approximate list:—

Cortinarius (Phleg).	triumphans, *Fr.*
Cort.	claricolor, *Fr.*
Cort.	sebaceus, *Fr.*
Cort.	varius, *Fr.*
Cort.	cyanopus, *Fr.*
Cort.	anfractus, *Fr.*
Cort.	multiformis, *Fr.*
Cort.	glaucopus, *Fr.*
Cort.	calochrous, *Fr.*
Cort.	fulgens, *Fr.*
Cort.	fulmineus, *Fr.*
Cortinarius (Myx).	elatior, *Fr.*
Cort.	mucifluus, *Fr.*
Cort.	Riederi, *Fr.*
Cortinarius (Ino.).	albo-violaceus, *Fr.*
Cort.	Bulliardi, *Fr.*
Cort.	pholideus, *Fr.*
Cortinarius (Derm.)	ochroleucus, *Fr.*
Cort.	caninus, *Fr.*

Cort.	anomalus, *Fr.*
Cort.	miltinus, *Fr.*
Cort.	sanguineus, *Fr.*
Cort.	cinnamomeus, *Fr.*
Cort.	infucatus, *Fr.*
Cortinarius (Tela). .	bulbosus, *Fr.*
Cort.	torvus, *Fr.*
Cort.	scutulatus, *Fr.* ?
Cort.	armillatus, *Fr.*
Cort.	hinnuleus, *Fr.*
Cort.	paleaceus, *Fr.*
Cortinarius (Hydr.) .	subferrugineus, *Fr.*
Cort.	castaneus, *Bull.*
Cort.	erythrinus, *Fr.*
Cort.	decipiens, *Fr.*

The above is certainly not a list to be despised for a single locality, during four days, of which, of course, some were rainy.

We cannot report so well of other genera, and know little of interest to be recorded.

The Hackney Microscopical and Natural History Society made a successful Saturday afternoon excursion to the Chingford side of Epping Forest on the 13th of October. A record was kept of upwards of 100 specimens of Hymenomycetes seen and determined, many of which of course were common species. Amongst the rarities was a very peculiary variety of *Boletus*, somewhat intermediate between *B. granulosus* and *B. bovinus*, and scarcely referable to either, which has been called *B. granulatus*, var. *tenuipes*. Besides this was *Ag.* (*Pleurotus*) *corticatus*, *Ag.* (*Pholiota*) *terrigenus*, and *Ag.* (*Amanita*) *spissus*. Several specimens of *Ag.* (*Clitocybe*) *cerussatus* were found, and proving agreeable to the taste whilst raw, were cooked and eaten, thus adding another, and an excellent one, to the list of edible species.

The Hertfordshire Natural History Society held their annual Cryptogamic meeting at Watford on Saturday, the 27th October, when a considerable number of species were added to the county list of Fungi, the complaint of the residents being that there were "fewer fungi in the woods this year than for many preceding years." Perhaps the most interesting specimens found were some very large *Ag.* (*Amanita*) *excelsus* and a cluster of *Ag.* (*Hypholoma*) *storea*.

This completes our brief record of the Fungi Forays of 1883. No frost, in the neighbourhood of London, occurred till the 13th of November, the Fungus season has this year been prolonged later than usual.—*From Grevillea.*

THE MICROSCOPIC STUDY OF FIBRES.

By James Cheyney.

THE Microscope in the Dye-room.—The delicacy of the processes of dyeing, and the ease with which slight differences in the dyes or in the waters supplied to the dyer modify the final results, render any method of examination which gives correct information of defects and their mode of occurrence great value. In this field no other instrument will compare with the microscope.

To test the injury inflicted upon wool by various washing fluids, we employed a little accessory to the microscope, which enabled us to keep the fibre in the field of the microscope for any required time, while it was still immersed in the fluid.

The same instrument, disregarding the weighing attachment, is admirably adapted for use in examining the effects of dyes. Our object here is, first, to examine the character of the results of the dye upon the fibre; and, secondly, to perform any special operations in the art desired under the field of view of the microscope, so as to find at what stage of the process defects, if such are found, arise. It is only possible in so vast a field to indicate methods of working, illustrating them by a few well-known cases.

The Marks of Perfect Dyeing.—We must first become familiar with the microscopic appearances in standard or perfectly dyed examples. Let us take, for example, one of the best of the beautiful, glossy sateens, which are so justly admired, and study it carefully. It will bear study in many ways, but we will keep our own point in view. Cutting off a piece about half an inch square, and fraying off the threads on each side till the ends are well exposed, soak the piece for ten minutes in a mixture of glycerine 3 parts, water 7 parts, and warm it gently, so as to expel the air from the tissue. Now place it upon the lower plate of "the compressor," and dropping over it several drops of the glycerine mixture, lower the thin glass upon it, and place it on the microscope, with a power of from 150 to 200 diameters. We find the fibres to be exceedingly perfect ones, not broken or split up by carding as the fibres of so many cotton goods are, but they are even and fine throughout. The colours are imbodied evenly in these fibres, appearing to be incorporated into the fibrous substance, and not deposited in grains or irregular masses within or upon the fibre. No manipulation will suffice to shake off any loose dye from the fabric, and the fluid in which it is soaked is neither tinted nor rendered dusty by loose fragments. Whatever may be the colours of the fabric they are all equally free from this loose deposit, and are all imbodied into the

substance of the fibre instead of being a precipitate upon or within it.

We may accept this as one type of perfect dyeing upon cotton fibre.

But some fast dyes are not thus incorporated into the tissue, and we will take a genuine red yarn of this class, and, soaking it as before, note the results. Along the centre of the fibre runs a deep red band which, on examination with a high power, shows a granular appearance. The body of the fibre is but slightly tinted, and some fine granular deposits are found here and there on the outside of the fibre, but they adhere so closely they cannot be washed off without considerable friction. As the dyeing is principally effected by the central or internal deposit, and no dust comes off in the fluid, nor does any colour dissolve away to tint the latter,—this is clearly a perfect dye of its kind.

Marks of Imperfect Dyeing.—If, on the other hand, we take up one of the cheaper red yarns, named in imitation of the last, and soak and examine it as before, we will find the following results:—

The central line of red, instead of being continuous, is broken up into coarse patches, while the outside of the fibre is covered with small grains of colour. These last fall off rapidly in the fluid, which is soon filled with them, and the colour also dissolves and tinges the fluid. This, evidently, is not well mordanted.

Again, we take one of the even body dyed cotton yarns of the new processes, and, steeping it a short time in our fluid, examine as we have done the others. There is no deposit on the outside, and no granular deposit in the centre of the fibre, but the tint, which is at first brilliant and clear, begins to grow paler rapidly, and in a short while tints the glycerine mixture, at first faintly, but soon deeper and deeper, until the colour has in great part left the fibre and become diffused through the glycerine. Evidently not well mordanted.

The Location of Defects.—But, supposing a defect to be found, in what way can the microscope aid us in remedying it? If it did no more than locate it, or show us just in what the defect consists, it would be a great gain. If we found, for example, that a tin-mordant, with a certain dye, should throw down a pure tint in the fibre, uniform and clear, and our fibre shows with this mordant and dye irregular and uneven granular deposits of colour not pure in tint, there is a mistake to be rectified. If our alum should give us, with a certain dye, an even fine-grained deposit, and we find a coarse, uneven one, we see a defect to be overcome. But it does more than this. It gives an opportunity, by using the warm stage and the compressor, to perform each operation of our dyeing, from first to last, under the microscope, We may watch appearances and temperatures, and note the point at which any special defect

arises. By a few trials at this point, we may succeed, by varying our mordant or the bath, or both, in getting rid of the trouble, and obtaining perfectly correct results. And it is this absolute power which the instrument gives of viewing the successive steps of the work under a strong light, and with a high power ; and of seeing, just at its beginning, anything which causes a blemish, and the power further to see when and how the trouble is removed, that renders it here most practical.—*Textile Record of America.*

PUBLIC AQUARIA.

IT is said that when King Henry VIII. inquired of the Abbot of Abingdon what was the depth of the sea, the learned Abbot answered, "A stone's throw, an't please your majesty's highness." We could answer the question to-day with rather more accuracy. If we throw a stone overboard in the deep sea, it would take half an hour for it to reach the bottom. About 150 miles from St. Thomas, the depth is about five miles and a half. The topography of the sea-bottom is being very carefully studied, and the dredge and trawl are sent down to bring up specimens of animal and plant life from the submerged plateaux and valleys. Strange and beautiful forms of life are found in the sea, of which few persons have any conception. No description can convey an adequate idea of the anemones with their spreading tentacles and delicate colors. The white coral skeletons in cabinets give no hint of the beautiful living creatures that formed it. The forms of the animals that live in the water are so strange and varied, and so different from anything that we are familiar with in daily life, that they possess a special interest whenever they are exhibited, either living or dead.

For this reason many attempts have been made to establish large marine public aquaria, where such creatures could be kept alive in glass tanks, and studied at leisure. The history of such efforts has been discouraging. Very few public aquaria are now in successful operation. Of these, scarcely half a dozen are famous. Nevertheless, the writer is firmly convinced, after long consideration of the facts, and a careful study of the causes leading to success or failure, that an aquarium can be established in almost any one of our large cities, such as New York, Boston, or Philadelphia, which would be a permanent attraction, and, if not quite profitable as a mere business investment, it could be made self-supporting from the beginning.

At first sight it may seem scarcely appropriate to the pages of this magazine, to advocate at length a scheme which appeals more to naturalists in general than to those who are especially engaged in microscopical studies. Yet, viewed broadly, from the standpoint of the scientific man, who draws knowledge from every source, and pleasure from every work of nature, and who sees in every enterprise tending to reveal the handiwork of nature something to educate and elevate his fellow man, the subject is not inappropriate to these pages.

The venerable naturalist and collector, the Rev. Henry H. Higgins, to whose earnest labour is largely due the arrangement of the specimens of invertebrates in the free public museum of Liverpool—an arrangement which places that museum among the best in the world for purposes of public instruction—says: "A specimen without a history, or even without a name, that calls forth a genuine exclamation 'How beautiful!' fulfils a noble mission, especially when the observer is a child or young." This is the expression of a man who has devoted the best years of his life to the cause of public instruction. It carries with it, therefore, the weight of experience—experience in the noblest work of life, that of an earnest effort to instruct and elevate mankind.

A labouring man from the north country, visiting the museum, was struck by the beautiful forms of the madrepore corals, and, calling to his wife, he exclaimed, "They be sea-crystals, all different sorts of crystals as grow in the sea." Had he stopped to read the explanatory cards he would have known that they were produced by the growth of beautiful animals. But it was enough for him that they were beautiful; and by his sincere appreciation of that one characteristic, his mind was elevated, be it ever so little, above the plane of sordid existence.

If skeletons and shells can thus impress the untutored mind, how much greater must be the influence of the sight of the living animals which have mysteriously formed them! Herein is the value of an aquarium as a means of instruction. Tell a child that the shells he plays with were once the houses of living animals, and he will wonder what kind of animals could live in such coiled and irregular shells. This is an example chosen from our own experience. How clearly the writer remembers the almost incredulity with which he first heard that animals lived in the shells—how could such a large animal as would fill the interior of the shell be held in his hand, crawl in and out through such a small doorway! The idea that the shell was carried about on the animal's back, was never thought of. Even now we might find one of the identical shells that so aroused our wonder in the days lang syne. Such are the strange notions of childhood; and the teacher who is most successful is that one who can not only direct the thoughts of

the young, but also detect the channels through which they are most likely to run wild. Show the child a living gasteropod, and then all will be clear to him; for he will see at a glance how the animals live and move about and eat. So much for an object-lesson from the pond or garden-snail. A simple one, indeed, but perfectly adapted to the needs, as well as to the capabilities, of a child. Not for a child alone, for probably not ten out of a dozen older persons in the city through could tell where the mouth or the eyes of a snail are located.

To such persons a well-stocked aquarium would reveal almost a new world of wonders. Who can foresee the influence it would have upon the young and active minds of school children, by causing them to look deeply into the book of nature broadly opened before them, and seek, first for the strange and beautiful, afterwards for the origin and reason and significance of it all! Everyone knows how people flock to a menagerie or zoölogical garden when there is a curious or unfamiliar animal to be seen. An unwieldly hippopotamus, or an ugly, ferocious beast of any kind, will be sure to attract hundreds of persons. If people retain their interest in the wild animals after having seen them many times, how much more must they find to interest them among the varied inhabitants of the ocean!

To the naturalists, a large and properly conducted aquarium would be of great assistance. Few persons are aware of the many valuable scientific observations already derived from studies at large aquaria, to say nothing of the great amount of work now in progress at sea-side laboratories, which does not now concern us. From a scientific stand-point, public aquaria are deserving of support. I think it was Prof. Edward Forbes who once wrote: "The naturalist whose acquaintance is confined to preserved specimens in a cabinet can form but a vague idea of the glorious variety of Nature, of the wisdom displayed in the building up of the atoms of matter to be the houses of life and intellect."

To take another view of the matter, some allusion may be made to what has been accomplished in solving problems that have long puzzled naturalists. A very choice morsel for the palate is the white-bait, a small, delicate fish, about an inch in length, which is sold at an extravagant price in the English markets. It was long an undecided question among naturalists what these little fishes became when they attained their growth. It was supposed they grew into the herring, *Clupea harengus*, but owing to their peculiar habits no one could prove the relationship. Prof. Saville Kent, however, succeeded in keeping them alive for eighteen months in the Manchester aquarium, and was thus enabled to prove that the white-bait, *Clupea alba*, did develop into *Clupea harengus*. Such an observation could not have been made except under the

exceptionally favourable opportunities afforded by large tanks supplied with thoroughly aerated, constantly running water, which can only be found in aquaria established for purposes of exhibition.

At the celebrated Brighton aquarium, which is one of the most successful in England, for it has paid well, another disputed question has been settled in an interesting manner. It was supposed by some naturalists and fishermen that the ova of the cod and the whiting were deposited on the sea-bottom, and if so it was feared that trawling would destroy the spawn, and thereby diminish the product of the fisheries. Experiment proved, however, that the eggs of both those fish would not sink, but floated on the surface of the water. At the same aquarium the rapidity of the growth of the salmon has been studied. Not to weary the reader with other facts of this kind, probably enough has been said to indicate the utility of large aquaria for purposes of scientific investigation.

A few words now concerning the principles which determine success in maintaining the necessary conditions for life in aquarium tanks. It is needless to review the gradual progress of knowledge concerning aquaria. It is well known at the present day that if the proper balance of animal and plant life be maintained in an aquarium, the products of respiration of the animals are removed from the water by the growing plants, and oxygen is given out in return. Although this perfect balance can be readily maintained in small tanks at home, it is not practicable to depend on plant life to aerate the water on a large scale. In the year 1861 Mr. Barnum procured two living whales, some sharks, and one or two other inhabitants of the deep, which he kept for some time in tanks supplied with salt water from the river. This was the first public marine aquarium in America, and the method now in most favor for keeping animals alive in tanks is the same as Mr. Barnum then adopted, except that instead of pumping the water directly from its source, a supply is stored in underground cisterns, from which it is continuously pumped into the tanks and allowed to flow back into the cisterns. As the water flows from place to place, it becomes well aerated; and the same water may be used for years. In the Crystal Palace aquarium the exhibition tanks have a capacity of 20,000 gallons, while the reservoir contains 100,000 gallons more. It is a question whether such a large reserve supply is necessary, or indeed, whether any considerable volume of water besides what is in the tanks need be kept. There are two benefits to be derived from it, however, first, the temperature of a large volume in the cistern must remain tolerably constant throughout the year, and second, any disturbance in one of the tanks, causing the water to become foul or turbid, can be immediately remedied by drawing the water off into the large cistern, when it mingles with such a comparatively large volume that it can do no harm. The loss of

water at the Crystal Palace aquarium has been calculated as only about 2 per cent by leakage.

Although this article has especially considered salt-water aquaria, it is by no means intended to neglect the attractions of the inhabitants of fresh-water, but to enlarge upon this portion of the subject would require to extend the article to an undue length. Public aquaria have not generally proved financially successful. Even now we would not advise any one to start one with a view to profit on the investment. Nevertheless, there can be no doubt that with proper management the running expenses could be paid from the beginning, and after a while funds would begin to accumulate. But we do not advocate this as a business scheme. It is advocated as a means of instruction for young or old, the founders of which will be entitled to the gratitude of the many who will be benefited thereby. Aquaria have failed in the past because of bad management, due, we may say, to imperfect knowledge of the conditions of success, or, when established for purposes of gain alone, because the managers have endeavoured to draw the people after the manner of a circus menagerie. Those that have been managed economically have not failed.

Few places are more favourably situated for marine aquaria than New York and Boston, especially the latter city, lying so near the haunts of marine animals of great variety. But even in Chicago, were it not for the difficulty of keeping the more delicate animals alive during the long journey from the sea-board, a marine aquarium could be maintained as perfectly as on the coast. Even such difficulties can be overcome, but not without considerable expense.

Now that the experience of the last ten years has shown how an aquarium should be managed, may we not hope to see in America, one that shall rival those of England and Europe? It requires no costly edifice, nor a very great outlay in fitting up. The cost can be very accurately calculated. But let whoever undertakes the task not go at it blind-fold. No mere business man can run an aquarium. It requires experience and knowledge which cannot be obtained from books alone. There must be a good naturalist at the head of it, who is deeply interested in his work, or the result will surely be disappointing.

ON "OPTICAL TUBE-LENGTH."

By Frank Crisp, V.P.L.S., Sec. R.M.S.

(Read 14th November, 1883.)

IT is not a little strange that at this late period in the development of the Microscope, an element of capital importance both from a theoretical and a practical point of view should have been left entirely unconsidered, and indeed unknown; and the fact that it is so, illustrates the disadvantage which English-speaking microscopists have always been under in having no text-book dealing with the theory of the Microscope.

In a letter written more than a year ago in reference to the Table of Magnifying Powers published in the Journal, Professor Abbe called my attention to the erroneous notions which prevailed on the subject of the magnifying power of the Microscope, and which he had been the first to clear up,* and I ought then to have published the explanation now given here, but the pressure of other engagements diverted my attention, and I confined myself to explaining the matter verbally to those who attended the meetings. Finding, however, that the Committee on Eye-pieces of the American Society of Microscopists have been misled by the Table in question, it is obviously desirable not to delay the explanation any longer.

Microscopists have always recognized that the length of the tube of the Microscope is a factor in determining the amplification of the image, that the amplification is generally greater with a 10 in. tube than with one of 6 in.; and that we obtain an increase of power by pulling out the draw-tube. Here, however, all exact notions as to the function of the tube-length have practically stopped, so much so that there has not been any agreement even as to how the length of the tube is to be measured, whether from the front or back lens of the objective to the field lens, the diaphragm, or the eye-lens of the eye-piece.

In particular, no view of tube-length has been held which would explain the following apparently paradoxical statements:—

That two objectives of precisely the same focal length used with the same tube and the same eye-piece may nevertheless give different magnifying powers.

That two objectives of different focal lengths used with the same tube and eye-piece will not give mangnifying powers in proportion

* Professor Abbe also communicated it to Dr. Dippel, by whom it was embodied in the last edition of 'Das Mikroskop,' 1882.

to their focal lengths: thus a 1-2 in. will not necessarily give double the power of a 1 in.

Conversely, two eye-pieces will not magnify in proportion to their focal lengths, though used with the same tube and objective.

Indeed, the true magnifying powers may differ from the powers which would be obtained on the ordinary assumptions by more than 100 per cent., and Prof. Abbe records the existence of objectives (of somewhat exceptional construction it is true) which exhibit this paradoxical behaviour: that one of longer focal length amplifies much more than one of shorter focal length; that one gives the same amplification with a long and a short tube, and that one gives a higher amplification with a short tube than with a long one.

What then is the explanation of these paradoxes?

The explanation is not to be found in any question of the length of the objective or eye-piece, or the character of their respective settings, but depends upon the fact that hitherto microscopists have regarded the outside only of the tube and have left out of consideration the optical action which goes on within it.

To properly understand the matter it will be necessary to consider the principles on which the action of the Microscope in regard to magnifying power is founded.

The magnifying power of a lens depends of course upon its focal length and varies inversely with it; the ordinary mode of obtaining the power being to divide the distance of distinct vision l (assumed as 10 in.) by the focal length, or expressing it by a formula $N = \frac{l}{f}$ Thus if the focal length f of an objective is 1-8th in. $10 \div \frac{1}{8} = 80$. The same applies to the action of the Microscope as a whole, that is with eye-piece and objective combined; when we have determined its focal length we similarly obtain its magnifying power.*

We have therefore to ascertain the proper mode of determining the focal length f of the entire Microscope, having given the focal length f^1 of the objective and the focal length f^2 of the eye-piece.

The usual assumption hitherto has been that f is determined by multiplying f^1 and f^2 together and dividing by the length of the tube 10 in., or

$$f = \frac{f^1 f^2}{10}$$

* The quotient obtained by dividing 10 in. by the focal length gives the linear amplification of an image—real or virtual—which is projected by an objective to a distance of 10 in. *from its posterior focus*, and not from the objective, as has been so commonly assumed.

so that if we had an objective of 1-8th in, and an eye-piece of 2 in. the focal length of the Microscope

$$f - \frac{\frac{1}{8} \times 2}{10} = \frac{1}{40}$$

A Microscope of a focal length of 1-40th in. would magnify 400 times, so that if this method of arriving at the focal length of the Microscope were correct, we should only have to multiply the power of the (1-8th in.) objective (80) by that of the (2 in.) eye-piece (5) to have the total magnifying power (400), the brass tube being assumed to be constant at 10 in.

The fallacy of this method lies in the fact that the true formula is not

$$f = \frac{f^1 f^2}{10}, \quad \text{but} \quad f = \frac{f^1 f^2}{\Delta},$$

Δ *being the distance between the posterior principal focal plane of the objective, and the anterior principal focal plane of the eye-piece*, or, as Prof. Abbe terms it, the rational or *optical tube-length*, in contradistinction to the mechanical or physical length.*

As Δ is the divisor of the fraction which represents the focal length, the latter is of course larger or smaller according as Δ is smaller or larger, that is, it varies inversely as Δ; and as the magnifying power is inversely to the focal length, the magnifying power varies directly as Δ, which is therefore seen to be a fundamental factor of microscopic amplification.

We can now see how it is that two objectives of the same focal length may yet give differing magnifying powers with the same tube and eye-piece. By the different methods of construction adopted by their makers, the focal plane of the one objective may be further off the back lens than is the case with the other. The distance Δ between the focal planes of the objective and eye-piece will be correspondingly diminished, and the focal length of the whole Microscope increased. The magnifying power will, therefore, be diminished.

Again, take the case of two objectives of say 1-8th in. and 1 in. focal length used with the same eye-piece (2 in.) and tube. If the distance Δ remained constant, say 10 in., the total focal length would vary with that of the objectives,

$$f = \frac{\frac{1}{8} \times 2}{10} = \frac{1}{48}, \quad \text{or} \quad f = \frac{1 \times 2}{11} = \frac{1}{5}$$

* The principal focal planes are the planes passing through the point on the axis in which parallel rays coming from the *opposite* side of the lens are brought to a focus. "Anterior" and "posterior" are used in reference to the direction in which the rays come to the observer.

But the posterior focal planes of the two objectives, instead of coinciding, may have different positions, every variation producing of course a change in the value of Δ. With the 1-8th in. objective the posterior focal plane may be very near the back lens, and we have a long Δ; with a 1 in. objective its posterior focal plane may be further from the back lens (higher up the tube), and we have a diminished Δ. We might have with the 1-8th in. objective $\Delta = 10$ in., and a power of $(80 \times 10 =)$ 800, but with the 1 in. objective we should not have $(10 \times 10 =)$ 100, or a total power in proportion to the powers of the objectives. Δ might be 8 in. only instead of 10 in., and the total power would be only 80.

The converse case of different eye-pieces with the same objective is similarly explicable. The anterior focal planes of the eye-pieces may be at different points of the tube, and we shall have a varying Δ.

As to the general character of the variations in Δ, it may be noted that the position of the anterior focal plane of the eye-piece does not vary much in the Huyghenian form; a substantial difference is, however, found in this respect between the Ramsden and Huyghenian, the former having its anterior focal plane at some distance below the field lens, and the latter above it. With the objective, however, a very wide range is possible. Its posterior focal plane may be (1) some distance above the last surface of the objective; (2) close to this surface outside or within the objective; or (3)—though a more exceptional case—as a virtual focus below the stage or even below the table. Practically, however, with objectives of ordinary construction, the difference in position of the posterior focal plane is not great with powers higher than 1-2 in., and it is only when we come to the lower powers that the difference is a substantial one.

Greater differences in the power will also be found with short tubes than with long ones. With a 10 in. tube a difference of 2 in. reduces the 10 to 8, but with a 6 in. tube from 6 to 4, quite different percentages of variation.

The process, therefore, of multiplying together the powers of the eye-piece and the objective to obtain the focal power of the Microscope is a fallacious one, as it supposes a constant tube-length; whilst, as we have seen, the true tube-length varies with the different objectives and eye-pieces used.

To determine the power of the Microscope from the powers of the eye-piece and objective, it is necessary, in addition, to know the position of the focal planes of each of the latter. How these may be readily determined must be deferred for a subsequent occasion.—*Journal Royal Microscopical Society.*

MICROSCOPIC TEST OBJECTS.

By E. M. Nelson, in the *English Mechanic.*

HAVING worked at these objects for some years, and having also kept pace with the times in objectives and apparatus, I give the results of my experience. 1st, the total abolition of oblique illumination if one wishes to see the true structure of an object; 2nd, object mounted dry on cover.

I use a P. and L. achromatic condenser, accurately centred to the optic axis. The edge of the flame of a paraffin lamp, with ½in. wick, exactly focussed on the object, without bull's-eye or mirror. This illumination, with a P. and L. oil $\frac{1}{12}$, N.A. 1·43, easily resolves *A. Pellucida*, dry on cover, with direct light—*i.e.*, without slot or stop.

If *S. gemma* is examined by this means, the hemispherule theory is at once exploded, and the true structure (which is far more beautiful) is revealed. It is something like a most delicate skeleton leaf. This, however, is very difficult for a beginner. The *P. formosum* is, perhaps, the best one to try first. Work away at that until the hemispheres, which are so easily seen, give place to a *square* grating! To see this, with a ¼, N.A. ·74, will severely test the lens and the observer's manipulative skill. A coarse *Nav. lyra* and a *Tryblionella punctata*, both with square apertures, are very easy. If the objective is much out of correction, the square apertures will blur round. The next one to try is *P. angulatum.* In this, a fracture should be seen to distinctly pass *through* the apertures. The apertures will take a rose tint if the glass is properly corrected.

It is manifestly absurd to test an objective by fine diatoms seen with oblique light, for only a small portion of a narrow marginal zone of the objective is used. The central, and by far the more important, part of the glass might be stopped out.

By the central illumination, however, the whole of the objective is used; the centre by the dioptric beam, the margin by the diffraction pencils. In former days one used to hear this sort of thing said: "This $\frac{1}{12}$ is a beautiful diatom glass." "This $\frac{1}{10}$ is splendid on 'Podura,' but not good at diatom resolving." (What a fine thing for the opticians! One had to buy two glasses, one for Podura and the other for diatoms). The explanation is very simple: For Podura a glass must be good in the centre, and for diatoms, with oblique light (the only light used in those days), good in the marginal zone. So then the $\frac{1}{10}$, which was good for Podura, and the $\frac{1}{12}$ for diatoms, could neither of them have been thoroughly corrected from their centres to their margins. I have a

glass in my collection which is very fair on Podura when the screw collar is in one position, and also is a good diatom resolver with its collar in another position; but when all its zones are tried at *once*, by the direct illumination, it utterly breaks down.

With regard to *A. pellucida*, the *strongest* resolution is obtained with P. and L's vertical illuminator. The long striæ can only be seen by this method. Spurious long. striæ may be easily seen; but the true lines are very difficult, and may be estimated to be 120,000 to the inch at the lowest. The transverse I have counted repeatedly, and find them, in Van Heurck's specimens, very constant at 95,000 per inch. The best picture of the trans-striæ is obtained with oil imm. $\frac{1}{12}$ N.A. 1·43, or oil imm. $\frac{1}{23}$ N.A. 1·38, and P. and L.'s oil immersion condenser, *used dry*, with single slot, edge of flame direct, valve being dry on cover. The lowest angled glass, with which I have seen the trans-striæ, is a water imm. $\frac{1}{16}$ N.A. 1·08, and the lowest power $\frac{1}{4}$, N.A. 1·17.

NORTH OF ENGLAND MICROSCOPICAL SOCIETY.

THE last monthly meeting of this Society was held on the evening of Tuesday, the 4th inst., in the patent room of the Literary and Philosophical Society, Newcastle-upon-Tyne; the President, Mason Watson, Esq., occupied the chair. The Rev. W. Johnson, of Hartlepool, sent a description with illustrative slides and drawings of a Lichen found in Cumberland, and new to Great Britain; an abstract of which is appended:—

"*Sirosiphon saxicola* (Nag) is a small byssaceous lichen found growing on damp rocks near to Ennerdale Lake, Cumberland, 1881. It is black when dry, and thinly scattered on the rock in a dark crust. The filaments are very minute, entangled and depressed; when magnified they are seen to be variously branched and somewhat mammillose. The cellules are in single series, doubling in the older filaments, clearly defined, and roundish in the young branches; the vagina is fuscous and narrow; fruit not seen."

The Hon. Sec., Mr. M. H. Robson, read a short paper, noting the detection of *Alcyonella stagnorum*, *Nitalla* (probably) *flexilis*, and *Cystopteris dentata*, new to this district, and enumerated a list of other objects, for which new habitats may be claimed.

Mr. John S. B. Bell, C.E., gave a description of a new form of Warm Stage, and adaptation of hemispherical lens for resolution of Diatoms; an apparatus for maintaining the slide at any temperature

from the temperature of the room up to 100 degrees. It consists of a mahogany slide 3″ + 1½″ + ¼, with a flat groove $\frac{1}{16}$″ deep for the ordinary glass slide to lay in. In the centre is a round hole one inch in diameter, which encloses a copper ring, made by bending No. 16 wire into a ring slightly less than the hole, and giving it one twist leaving the two ends to pass longitudinally through the stage when they are twisted together, leaving a single wire at the end about one inch longer, with the end curled round. The stage is heated by a spirit lamp held to the twisted wire, and when the required temperature is reached the lamp is moved back along the wire to a point that will just maintain the temperature. The room was 62° F. ; the slide was heated to 82°, and the temperature kept stationary. It was then heated to 100° F., and kept stationary for half-an-hour. In this arrangement the heated wire is isolated from the stage, and from the glass slide by means of the wood in which it is placed. For purposes of crystallisation, the growth of Torulæ, Bacteria, and kindred subjects, this form of apparatus will be found simple, cleanly, and easily worked.

Mr. Bell next exhibited a form of stage condenser for illuminating diatomaceæ ; the principal point is it will suit any microscope which is not fitted with substage arrangements. It is simply an addition of a shutter to the hemispherical lens : this shutter is easily removable, as it is held to the brass seating of the lens by friction, it can be fitted to any lens, though for the best effect the lens should not be less than ½ an inch in clear aperture, and slightly less than hemispherical, say of a thickness equal to four-fifths the radius. The shutter is similar to that used by Messrs. Powell and Lealand. For their substage condenser there is a central aperture and two side ones at right angles to each other, which can be used at pleasure. This form gives very satisfactory results, and *Navicula rhomboides* has been resolved with $\frac{1}{16}$ by Swift, equal to what can be done with same frustules and Powell and Lealand's apparatus complete.

BIRMINGHAM NATURAL HISTORY AND MICROSCOPICAL SOCIETY.

AT the meeting of this Society on Tuesday, December 4th, at the Mason College, the President (Mr. T. R. Waller) in the chair, Professor W. Hillhouse, M.A., F.S.L. (Professor of Botany and Vegetable Physiology at the college), delivered a lecture on some recent discoveries which he has made in the structure of

plants, and which will ultimately prove of the very greatest importance. By these discoveries, in which several botanists, notably Mr. W. Gardiner, B.A., of the Jodrell Laboratory at Kew, have taken part, a number of the old ideas about the cell structure of plants are completely overthrown. Of late years gradual advances have been made in the direction which will be more particularly specified further on, and several eminent men have prophesied on *a priori* grounds that the discovery which is now made would come to pass. But still the honour of actually proving the fact belongs alike to Professor Hillhouse and Mr. Gardiner, who arrived simultaneously and independently at similar results, thus adding another instance, to those in which science already abounds, of two minds working at distant places in the same direction, and at the same time.

Professor Hillhouse's lecture was of a preliminary character, and was devoted to clearing the ground for the full comprehension of the importance and meaning of his results. All physiologists teach now that animal and vegetable organisms alike consist of minute sacs or bags of some substances, within which is contained fragments of that part of the organism in which its life essentially resides, to which they give the name of "protoplasm;" the enclosing membrane with its contents forms what is known as a "cell." Between the cells of animal and vegetable organisms, however, they point out a number of differences, of which we need only mention one. That is, that while animal cells are separated from one another only by a layer of protoplasm, slightly different in its character from the protoplasm which it encloses (and known as "cement-substance"), the cells of a vegetable organism, on the contrary, are surrounded by a layer of a substance totally unlike protoplasm, known by the name of "cellulose;" and it was always taught that the protoplasm of the cell was completely shut off by the cellulose membrane from all communication with the protoplasm of the neighbouring cells. In fact, each vegetable cell was considered as an isolated individual. It is this last doctrine that Professor Hillhouse's observations have finally disproved. In several which he has examined, of a sufficiently diverse character, taken in connection with Mr. Gardiner, to afford a satisfactory basis for induction, he has shown that the protoplasmic contents of neighhouring cells are connected by delicate threads of protoplasm, which pierce the cell wall by minute pores, and are continuous from cell to cell. They constitute, in fact, a sort of nervous system, but in all probability are confined to those kinds of cells in which the cell wall is not extensively thickened.

The existence of such a connection could be prophesied on the following *a priori* grounds:—In the first place, there are found in many plants what are called "sieve-tubes," the walls of which are

pierced by obvious pores, and it has been proved that threads of protoplasm pass through these openings. In the second place, it is now known that when the protoplasm of a cell is contracted by salt solution or other similar reagent, the ball of protoplasm which collects in the centre remains connected with the cell-wall by fine threads, which terminate in a knob, and, penetrating the cell-wall by a "pit" or depression to its "middle layer," are attached firmly thereto. Now these pits in contiguous cells are often opposite, so that between the knobs attached to the base of each pit there intervenes only the thin "middle layer"; the portion of this layer, which lies immediately between the bases of the opposing knobs, is seen to be streaked transversely in such a way as to suggest that minute threads of protoplasm pass from one knob to the other. Professor Strasburger, of Bonn, was convinced that "the difference between this case and that of the obvious apertures in the 'sieve-tubes' was only one of degree."

Again, it is known that an influence exerted upon one cell of a plant can be transmitted to the adjoining cells. For instance, if one leaflet of the sensitive plant be touched it approaches the opposite leaflet to which it is closely applied. The impulse is transmitted to the next pair of leaflets, from them to the next, and so on. It then reaches the base of the pinnule, from which it travels to the other pinnules of the same leaf, all of which close their leaflets in the same manner. The impulse then travels to the base of the leaf stalk, and the whole leaf drops down. Now, this and other movements of plants inevitably suggest the existence of some mode of direct communication between cell and cell, but it has been hitherto unknown how this communication was effected. It has been constantly denied that plants possess anything of the nature of a nervous system, but Professor Hillhouse's observations, and those of Mr. Gardiner, supply the gap, and show how the influence is transmitted.

Professor Hillhouse also gave a short *resumè* of the growth of our knowledge about protoplasm, and a few other ideas necessary to put the hearer into a position to understand the subject into which he will enter more deeply in a future address. The lecture was illustrated by a number of very skilfully prepared microscopic slides, which showed the chief points on which the lecturer had dilated. One especially, prepared from the filmy fern (*Todea*), the cell contents of which had been contracted twenty-four hours before by a ten per cent. salt solution, was very beautiful; it distinctly showed the radiating threads of protoplasm stretching from the contracted mass to the cell wall, along which nodes or lumps of granules were passing outwards with a steady motion, on their way back to reform the living protoplasmic membrane of the cell.

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Tubbs, Brook, and Chrystal, 11, Market-street, Manchester, to whom also all applications for advertisements should be made.

ALL matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, The Willows, Fallowfield, Manchester.

SOIREE. — The Associated Soirée of the Liverpool Learned Societies was held on Wednesday, December 19th, in the St. George's Hall.

ADVICE TO STUDENTS.—Dr. W. B. Carpenter, F.R.S., in accepting the post of Vice-President of the Carlisle Microscopic Society, replied in the following words, which we commend to the notice of all students :—

"I accept with much pleasure the office of Vice-President of the Carlisle Microscopical Society, for which you are good enough to propose me; and shall be very glad if any words of mine can help to give such a direction to the work of its Members as may prevent the 'power' of your Society from 'running to waste.'

"For this end it is extremely important, in my judgment, that Microscopists should first train themselves in the expert use of the instrument and its most important appliances; and should then devote themselves *especially* (I by no means desire *exclusively*) to *some particular study;* each selecting what his own opportunities and mental interests make him feel most suitable to himself.

"It was thus that my late friend and early pupil, G. H. K. Thwaites, who had taken up the study of *living* Diatoms at my suggestion—now 40 years ago—was enabled to discover the cardinal fact of their conjugation and production of a Zygospore. And if one-tenth of the time that has been since bestowed on the markings of their valves had been given to the study of their life history, our scientific knowledge of the group would have been greatly advanced, instead of remaining almost stationary. The continuous study of the life-history of the *Monads* by Messrs. Dallinger and Drysdale, which has given results of first-rate importance to Biological Science, is a recent example of what may be done by a combination of two (or more) qualified observers. And

I need scarcely point out to a body including many Medical men, what a wide field there now is in the study of *disease-germs*.

"As a qualification for that study, I should suggest the determination of the life-history of the *Yeast-plant*. For there is a strong reason to believe that what we know under this form is only an aberrant stage in the life of an ordinary *Mucor*; its cell-germs developing themselves in a very different mode, in a saccharo-albuminous liquid, from that in which they vegetate on an ordinary mould-producing surface. And while, on the one hand, it was long since observed by Mr. Berkerley that a *Mucor* may develop itself in a *confervoid* form in ordinary water, it is still an open question whether, if growing in an organic fluid, the same Mucor may not become the 'Vinegar Plant.'

"I have always, myself, been a believer in the great polymorphism of the 'saprophytic' Fungi; and I recently read at Southport, a paper on 'Disease-Germs from the Natural History point of view,' in which I argued that the extension of the same idea to disease-germs will account for many clinical facts observed by able practitioners of Medicine, which have hitherto received (in my opinion) far too little attention,—I mean, the occurrence of what have been called hybrid varieties of Exanthemata, or of forms of fever intermediate between Typhus and Typhoid, or the conversion of an endemic malarious remittent into a contagious fever.

"It is because the Microscope thus gives most important aid in the working out of some of the fundamental questions of Pathology, that I am most anxious to see Medical men training themselves to the right use of it."

We notice that several important papers appear on the programme for 1883-4 *viz.*: "Water," "The Salmon Disease," "Structural Botany," "Animal Tissues," "The Fertilization of Flowers by Insects," "The Microscope in Manufactures," "The Adulteration of Food," and "Microphotography."

FISHERIES EXHIBITION AWARDS.—We are glad to notice that Mr. Thomas Bolton, of 51, Newhall-street, Birmingham, has been awarded a gold medal for his general exhibition of Invertebrata. During the exhibition he has had on view many rare and beautiful organisms such as the fresh-water Jelly-fish, Young Smelts, Perch, Salmon, &c. There was also shown a new rotifer, *Asplanchna Ebbesbornii*, and the new worm, *Haplobranchus æsturina*, which Mr. Bolton discovered last year. The tenth portfolio of drawings will be ready in January.

LIVERPOOL MICROSCOPICAL SOCIETY.—The ninth meeting of the Session was held at the Royal Institute on Friday, December 7th, when Mr. A. T. Smith, Jun., read a paper on "The Anatomy of the Cockroach, and remarks on the mounting of its various

parts." At the conclusion of the meeting the usual Conversazione was held.

From North to South.—Many of our readers will remember Mr. Thomas Whitelegge, of Ashton-under-Lyne, who, about twelve months since, set out for South Australia, intending to settle there. He has lately written back to the old country, and the following is an extract from his letter :—

"There are plenty of microscopists here with splendid instruments, but many of them do nothing but dot Diatoms. I have not done much at Botany yet, although I have frequent rambles in the Bush, which abounds with the prettiest flowers I ever saw, and all imaginable shapes and colors. Snakes and lizards are very plentiful, so that you have to look out where you set your feet. Frogs are very prevalent and very pretty. One night I was startled by seeing something on the window, and there was a tree-frog (about the size of an English frog), on the glass adhering by its suckers to the window-pane, it moved about the glass quite comfortably for about half an hour, much to our amusement while we had our tea. I live in a nice locality for a Naturalist, some eight or ten square miles of bogs, swamps, and pools within a few minutes walk, or if I require the sea-side, I am about three miles from it. I can see Botany Bay from where I live, and can go by train for 6d., about eight miles.

"I have made the acquaintance of most of the scientists here, and am a F.L.S. and F.R.S., of N. S. W., Sydney. The Rev. J. E. Tenyson Woods gave me about 12 volumes of books, all bearing on Australian Natural History. He is a very great authority in all branches of Natural Science, well-known throughout Australia, having travelled nearly all over it. I have had several outings with him in search of infusoria. I have had good luck with my pond life studies here, having found a great many things not hitherto found in the colony. Two species of Polyzoa, *Plumatella repens* and *Fredericella sultana*, of Rotefera. I have added about six species to those already recorded; and have found four species new to science, I believe,—two species of Fresh Water Sponge, one hitherto only found in Queensland, and the other a new species, besides a lot of other microscopic things, which have not been observed here before."

Obituary.—Two microscopists have passed away from our midst: J. Lawrence Smith and Robert Tolles; the former a man who applied the microscope to every investigation he undertook; the latter leaves his monument by having handed to posterity some of the finest lenses the world possesses. *Hœc olim meminisse jubavit.*

Manchester Cryptogamic Society.—The fifth annual meeting of this Society was held on Monday evening, Dec. 17th, Dr. B. Carrington, F.R.S.E., in the chair.

Mr. Thomas Rogers, the honorary secretary, read the annual report. It reviewed the work done by the members, enumerating the new Cryptogamic plants which had been discovered in Britain by the members or their friends during the past year, the new European Continental species by corresponding members, a remarkable number of rare species that had been discovered in new localities, and some species which had not hitherto been found in fruit. The most interesting of the local species were *Mnium stellare* and *Gymnostomum calcareum*, found in fruit by Mr. Holt in Derbyshire. The literary work of the Society had been most pleasant and interesting. Mr. Cash had written the history of *Cinclidium stygium* as a British Moss, and papers on the earlier bryological works of Mr. William Wilson in Scotland, Ireland, Anglesea, Cheshire, and Lancashire. Dr. Carrington had contributed a large packet of letters containing correspondence of many of the best known cryptogamic botanists who lived in the earlier part of the present century with the eminent artizan botanist, Edward Hobson. These letters may some day appear in a complete form. It was also satisfactory to know that the president and vice-president, Dr. Carrington and Mr. W. H. Pearson, had issued Fasciculus III of the *Hepaticæ Britannicæ Exsiccatæ*.

The following were elected the officers of the Society for the ensuing year:—Dr. B. Carrington, president; Captain P. G. Cunliffe and Mr. James Cash, vice-presidents; Mr. T. Rogers, honorary secretary and treasurer; librarian, Mr. Pearson.

During the evening Mr. William Forster exhibited a splendid series of twenty-two varieties of the common *Polypodium vulgare*, from the fernery of J. M. Barnes, of Milnthorpe. Most of the specimens were remarkable, and many of them very beautiful. Mr. George Stabler sent specimens of *Lophocolea spicata* (Tayl), collected by the late W. Wilson, near Conway. This species has not hitherto been recorded as growing in Wales. Mr. Stabler also sent *Jung. Schraderi*, from a new locality by the river Lune, near Sedberg, collected by himself in October, 1882. Another rarity was *Bryum concinnatum* (Spruce) from the Pass of Llanberis, collected May, 1883. Mr. Cash exhibited specimens of *Andreæa sparsifolia*, which had been collected on Helvellyn, in September last by the Rev. C. H. Waddell. The thanks of the society were accorded to Dr. Braithwaite, F.L.S., for all the parts yet published of his *British Moss Flora*, and to the Royal Microscopical Society for their Journal and Proceedings.

Transparencies for the Lantern.—Mr. Chapman gives the

following instructions for producing transparencies from his dry plates. We have used these plates, and have much pleasure in reporting them easy to work and yielding excellent toned pictures:—They are combinations of Gelatine with Albumen, having a film of most exquisite fineness, capable of giving a variety of tints, from a dense black to the much admired tone of the *Continental Lantern and Stereoscopic Transparencies.*

For making Lantern Slides, supposing the negative is of a suitable size for the purpose, artificial light is the most convenient to work by, and the simplest method is transparencies by contact.

To produce these, put the negative in a printing frame and (in a room having none but a *non-actinic* light) place a prepared plate in contact with it, then expose the negative to an ordinary gas flame at say 3 feet distance for 30 seconds, after which return to the dark room and place the exposed plate in a *porcelain* tray, containing Chapman's FERROUS OXALATE DEVELOPER, and in a few seconds the image should appear. When all detail is out do not consider it to be fully developed, but turn the plate up from the solution and look at the *back* part of it; if the *mere* outline of the subject is only visible, lay the plate down again, and give it a minute or two more soaking; if on looking again at the back the *detail* is pretty well defined, the plate may be considered fully developed and will be ready to be taken from the developer, drained and RINSED in water, and then placed into another porcelain or glass tray, containing the *Fixing Solution*, as follows:—Sodium Hyposulphite, 4 ounces; Water, 20 ounces. Let the plate remain in this solution for, say five minutes, or until quite free from opalescence, then drain, and, without rinsing, transfer it direct into another tray containing the following solution:—Alum, 1 ounce; Water, 20 ounces. After being in this solution for *five* or *ten* minutes it may be put to soak, to thoroughly rid it of Hyposulphite and other salts, and the best way to do this is to put it into a conical or taper vessel—say a basin or deep saucer—of water, *face downwards*, and change the water occasionally. After a few hours, or a whole night's soaking in this manner it may be considered ready for the final wash before drying, and to obtain the most brilliant pictures it is well to give this in a running stream, and whilst the water is flowing over the plate to go over the *film* with a *soft* camel's hair brush, a tuft of *cotton wool*, or the soft inner surface of the fingers. Afterwards, let the plate be placed in a rack to dry, and this should be in a moderately warm room, and the precaution taken not to disturb the plate whilst drying, or possibly surface markings will be produced. When quite dry it is ready for varnishing.

The above simple directions are more for those who have had no experience in producing Transparencies. Those acquainted with the method of producing either enlarged or diminished pic-

tures per Camera require no instructions as regards the mechanical part.

Although in the above instructions the maker mentions his own *Ferrous Oxalate Developer* for these plates, he does not wish to intimate that no other mode of development will do. The plates may be developed by many of the Pyrogallic methods, and also the Ferrous Oxalate from the mixed solutions of Protosulphate of Iron and Potassium Oxalate, examples of which he here tabulates. He, however, begs to state that he considers the most pleasing colour that produced by his own special *Ferrous Oxalate Developer.*

Owing to the varying density of negatives, it is impossible to give the exact time for exposure. That given above is found to be right for a negative of medium density. A very simple plan, and one which often saves time, by allowing the worker to remain in the dark room, is to expose the plate by burning a wax vesta or common match over the negative, care, of course, being taken to have all sensitive plates protected during the operation.

THE ROYAL SOCIETY.—Professor Huxley's election as president of the Royal Society was confirmed at the last annual meeting. In his address Professor Huxley reviewed the society's work during the past year, and made touching references to the eminent men who have passed away. Referring to his election as president he said, "To a man like myself, who neither possesses, nor seeks, any other distinction than that of having done his best to advance knowledge, and to uphold the dignity and the authority of science against all comers, the presidency of this society is the highest dignity which he can attain, whatever else may befall him. But, as men of science, you know better than I can tell you, that there are things of more worth than distinction. I am within measurable distance of the end of my career; and I have looked forward to the time when I should be able to escape from the distractions and perturbations of the multitudinous affairs in which I have been so long entangled, to that student life from which the Fates have driven me, but to which I trust they may, for a little space, permit me to return. So that I am sure you will neither misunderstand me, nor mislike my directness of speech, when I say that, if it please you to believe that the interests of science, and of the Royal Society will be advanced by maintaining me in the very distinguished position which I at present occupy, I will do my best to justify your confidence; but if, as may well be, you think that some other Fellow of the Society will serve these interests better, I shall, with a light heart, transfer to him the honourable burden, which I have already borne long enough to know its weight."

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

No. 38. *FEBRUARY.* 1884.

MICROSCOPICAL EVIDENCE CONCERNING BLOOD CORPUSCLES.

FROM reports which have come to us through the newspapers we have very meagre knowledge of the circumstances under which a gentleman, well-known to the readers of this Journal, has testified in a recent trial that certain spots found upon a coat were produced by human blood. The evidence given, as we have learned from the published reports, was of a nature to influence the result of the trial, and as it seems to have been of a very positive nature we must presume that the attendant circumstances were such as to fully justify it, for, at the present time, it seems rash, to say the least, to venture very positive testimony founded upon measurements of blood corpuscles alone. In other words, if, in the case referred to, it were conclusively shown that the stains could not have been produced by the blood of some animal whose blood-cells are almost the same as those of man, then the microscopical evidence would be sufficient to prove the nature of the stain, and to distinguish the blood-cells from those of birds, etc. But, on the other hand, we have grave doubts, which are shared by many microscopists of experience, if the shape and size of the cells of dried blood, obtained from woven fabrics by soaking in mercuric chloride, or in any other way, are sufficiently characteristic to justify any positive evidence of their origin, based upon our present knowledge and experience.

We do not express the opinion that it is impossible to distinguish the different kinds of corpuscles in this way. On the contrary it seems very probable that it can be done. There seems to be great constancy in the average sizes of the corpuscles of different animals, and, providing a sufficient number of them are measured to get a fair average size, there is no doubt of our ability to distinguish different specimens of fresh blood with absolute certainty. But in dealing with dried blood, especially such as is dried upon cloth, or fibrous surfaces, the corpuscles are likely to be distorted, and it is

more difficult to get a fair average by measurements. Moreover, the shrinkage of corpuscles in drying may be greater or less under different circumstances, and its amount is not yet known with any certainty. In any case it must be very slight, to be sure, but in a matter of such vital importance it cannot be neglected by the scientific observer, until its amount is known for all circumstances. Granting the strong probability that the microscope does, under favourable circumstances, afford a means of positively identifying human blood, and distinguishing it from all other blood, we must still hold to the opinion that, until experience has shown such evidence to be sure and infallible, no scientific man is warranted in stating that a stain upon cloth is made by human blood, from the microscopical examination alone.

Doubtless in the particular case referred to the microscopist was fully justified in giving the positive testimony that has been reported, but we would particularly impress upon the reader the uncertainty of the microscopical evidence taken alone—it may be justly maintained that such evidence is, to say the least, not infallible. The subject having been brought before the public once more it seems desirable that microscopists should be reminded that the microscope cannot be regarded as infallible to distinguish human blood. It may be we are conservative about it. We are willing to believe that the microscope is capable of identifying human blood from the examination of a few dried corpuscles, but the proof that it is so seems to us not conclusive. If we err it is upon the side of safety; and to those readers who have had no practical acquaintance with such examinations we would say, do not be led into error by the apparent simplicity of the problem. If human blood can be identified with the microscope it can only be done with safety by persons of great experience in the examination and measurement of blood-cells.—*The American Monthly Microscopical Journal.*

SELAGINELLA: ALTERATION OF GENERATIONS.

By W. Stanley.*

In drawing your attention this evening to this family of Cryptogamous plants I do so for two reasons. In the first place, although it has most important botanical characteristics, it may be said to be very little known amongst botanists in general; and in

* A paper read before the Manchester Microscopical Society, Jan. 3, 1884.

the second place, in the development of its life history it shows, in a far higher degree than any other Cryptogam, the connection between Cryptogams and Phanerogams. The family is moss-like in appearance, mostly European, and belongs to the order Lycopodiaceæ, the name Selaginella (lesser Alpine Club moss) being a diminutive of Selago, the Lycopodium.

Only one species, *Selaginella selaginoides*, is found in England, on boggy ground, especially by the sides of small streams and ditches, and on wet rocks in mountainous districts.

Many of the foreign species are known in greenhouses, where they are cultivated for their bright metallic green colour.

The stems are dichotomously branched, and show in a very distinct manner what is absent in the Mosses and Hepatics, but present in the Ferns, Equisetums, and Lycopods: the fibro-vascular bundles.

These bundles are connected with the cortex by a very loose spongy tissue, so that they appear to lie isolated in a cylinder filled with air, and connected with the walls only here and there by parenchymatous cells.

The structure of the vascular bundle itself is always uniform. The woody portion, or xylem, consists of wider vascular cells in its inner, and of narrower vascular cells in its outer part; the less solid portion, the bust or phloëm possesses vessels, fibres, and parenchymatous tissue. The axial vascular bundle sends out ramifications into the branches and leaves.

The leaves are ligulate, placed in four rows, and are of different sizes; the lateral rows consisting of larger, the upper and under of smaller leaves.

This is a very marked feature, and enables us at once to separate it from Fissidens or Hypnum, two genera of mosses which it closely resembles.

Only one kind of spore is produced in Mosses, Ferns, Equisetums, and Lycopods, but in Selaginella, Pillularia, and also in Isoëteæ, a family of aquatic plants, which is the only one known among Cryptogams, in which the stem permanently increases in thickness, two kinds of sporangia are produced, Macrosporangia, in which are formed four large macrospores, and are found in the axils of the lower leaves. Microsporangia, in which a greater number of much smaller spores, the microspores, are developed, and are found only in the axils of the upper leaves.

The microspores are the Antheridia, and break up into a small number of cells, one of which remains unproductive, and may be regarded as an abortive pro-embryo, while antherozoids are developed in the remainder.

The macrospores, on the other hand, produce a pro-embryo or prothallium, which bears archegonia, opening outwardly,

and attached to the apex of the macrospore as a cup-shaped appendage.

After fertilisation by the antherozoids there is, for the first time, seen the production of a true embryo, which forms a filiform suspensor, a body which is wanting in other Cryptogams, but present in all Phanerogams, on which the young plant, with its growing point, ligule and two first leaves or cotyledons are developed.

For the first time also there appears in the spore along with the female prothallus, yet distinct from it, a mass of cells, which supply nutriment to the young and growing embryo. This is the endosperm of the seed of the Phanerogam.

That special feature of Cryptogamic life, the alternation of generations, first clearly defined by Hoffmeister in 1851, and commencing with the Zygosporeæ, in which the zygospore comprises the whole or asexual generation, reaches its highest development in the moss, gradually decreases in character through Fern, Equisetum, and Lycopod, until, in the Selaginella, the first or sexual generation is wholly comprised in the macrospore prior to becoming extinct, in the seed.

No fossilized traces of this family have been found, but two species of the allied family Isoëtes have been traced in the Miocene period.

In the course of further remarks Mr. Stanley showed how the classification of Cryptogams was mainly based upon the alternation of generations, and said that what we commonly called a moss was the sexual or first generation of the plant, producing organs of fructification, Antheridia and Archegonia, at the apices or on the leafy stems, the more highly developed capsule, with its spores, constituting the second or asexual generation.

In the fern the sexual organs are produced on the thallus, that portion of the plant with its true roots, stems, fronds, and fibrovascular tissue, and which we recognised as a fern, being the second generation, whose immediate function was the production of spores.

Although a great number of vegetating cells were generally called spores, the only true spore, capable of reproducing the complete life history of the plant, was the one resulting from sexual development; all other forms, such as the conidia or gonidia of Penicillium, the gemmæ of the moss, the bulbil of the fern, were the result of asexual development, and only able to give rise to the same generation of the plant on which they were formed.

In conclusion, he urged upon the young members of the society the study of some of the lower forms of Cryptogamia as necessary to the correct appreciation of many of the problems of higher plant, or even of animal life.

The paper was illustrated by a number of drawings and mounted slides of many of the species referred to, as well as by a selection of excellent slides prepared by Mr. William West, of Bradford, and kindly lent for the occasion by Mr. Thomas Rogers.

THE DETECTION OF ADULTERATION IN FOOD.

By C. M. Vorce, F.R.M.S.

MUSTARD.

THIS condiment, of so universal use, is probably never obtained strictly pure. The commercial article is the flour of the seeds of black mustard and white mustard mixed in a proportion that varies with the fancy or honesty of each manufacturer; the black mustard seeds furnishing the best mustard flour. To the mustard flour is added a large proportion of wheat flour, which serves to absorb and retain the abundant oil of the crushed mustard seeds, and it thus ranks as commercially pure. A celebrated English manufacturer of mustard uses 56 lbs. of wheat-flour to 112 lbs. of mustard-flour made from two parts of black to one part of white seed. This mixture is accepted as a very superior article of "pure ground mustard." In cheaper grades of mustard the proportion of wheat-flour is increased, the sifting out of the mustard seed husks is less perfect, rye-flour and other cheaper flours and corn meal are used, and various adulterants are added, of which one of the most common is turmeric, as it also serves to darken the yellow colour of the mixture when much starchy flour is used.

The mustard seed itself is also subject to adulteration, but not extensively, cheaper and harmless seeds being mixed with it, clove seed being the most common. The appearance of the mustard seed under the microscope is beautiful indeed, the surface is covered with a network of raised ridges enclosing shallow concave pits of rudely hexagonal shape, smaller and more irregular as they converge toward the micropyle of the seed, and containing a shrunken membrane-like layer of glistening white substance lining the sunken cavity, and in some places looking like a flat scale-like crystal lying in the pit, fig. 1. The shell or husk of a dry seed examined in turpentine or water, exhibits a rudely hexagonal areolation, and is apparently thickly punctured with small holes, and of a somewhat translucent red colour, like coloured horn. fig. 2, *a*. The seed-shell is lined inside and out by a thin, close, cellular membrane, the outer one with very much larger cells than

the inner, and having a dinted appearance, fig. 2, *c.* The inner membrane is entirely hyaline and glass-like, and when viewed on

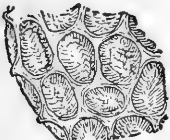


Fig. 1.

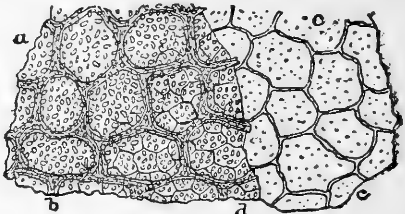


Fig. 2.

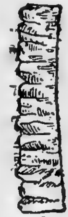


Fig. 3.

Fig. 4.

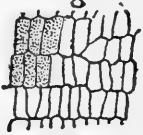


Fig. 5.

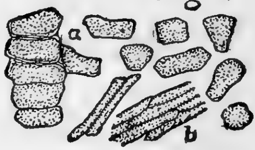


Fig. 6.

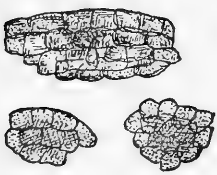


Fig. 7.

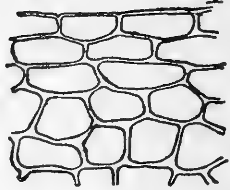


Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

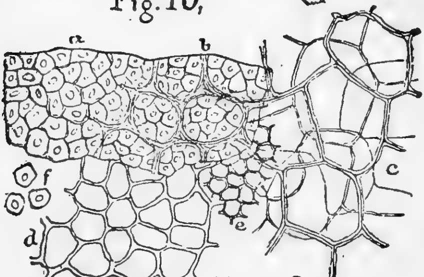


Fig. 12.

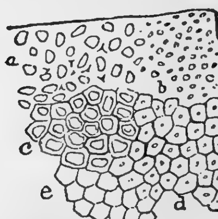
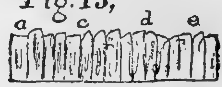


Fig. 13.



the inside of the dry seed-shell, by lieberkuhn, it appears to be the inner surface of the husk itself, composed of small irregular cells

having a sunken appearance and a bright blood-red colour. This inner membrane, in fact, adheres so closely to the shell as to almost universally follow its fracture, and so is seldom seen projecting from the broken edge of the husk as the outer membrane often is (fig. 2, *c.*), but on taking a seed that has been soaked for a day or so in water, or for a short time in dilute nitric acid, and squeezing out the cotyledon, the outer and inner membranes will be found swelled and softened, and can be scraped off with needles and examined separately. The inner one then appears as in fig. 12, *d*, and the outer one, much more swollen, as in fig. 12, *c*.

The structure of the substance of the seed-shell is very peculiar and complex, and more space than is at command would be required to describe it fully. Its appearance, so far as identification is concerned, is, however, distinctive and easily recognised. When untreated, the shell presents in water, turpentine, or glycerine, the appearance shown in fig. 2, *d*, when viewed with the inner surface uppermost: the ridges of the outer surface (*b*) are indistinctly seen through the shell, which appears to be composed of rudely hexagonal blocks containing, each, several of the small holes (*a*) with the large-celled, mottled or dinted outer membrane (*c*) projecting in places. The appearance of hexagonal blocks is caused by the cell-walls of the inner membrane, for when this is scraped off, no such appearance is seen (fig. 2, *a*). By soaking the shell in potash or acid it is made apparent that each hole is in a separate block, for these are clearly seen, fig. 12, *a*, and can be picked apart with needles, fig. 12, *f.* The seed-shell has, however, a lining membrane to these blocks, for in a piece treated in acid and picked apart with needles, not only the outer membrane, fig. 12, *c*, the inner membrane, fig, 12, *d*, and the separate blocks (*f*) of the shell are seen, but also a smaller-celled membrane, whose meshes correspond in size and shape to the blocks of the shell substance, fig. 12, *c*. In fig. 13 are shown the different appearances of the seed-shell under various treatments, and also in section.

In the preparation of ground mustard, repeated sifting is resorted to to remove the fragments of the seed-shell, called "husks," but minute fragments will be found in all samples if they contain mustard at all.

The cotyledon of the mustard seed is composed of elongated cells arranged in pretty regular rows lengthwise of the seed (fig. 4). In most parts these rows are very regular and the cells of even size (fig. 5). The cells of the radicle are long and slender (fig. 6, *b*). By chopping up with a knife the cotyledon of a soaked seed and crushing down the fragments in water, the separated cells will be seen (fig. 6). As seen in the mustard flour, the fragments of ground mustard will present small irregular lumps of cells, of

various sizes and shapes, opaque in the thicker parts, and with the cells filled with a minute granular substance (fig. 7). Some scales and cells of the outer membrane of the shell (fig. 11), are seen, some separate cells of the mustard seed (fig. 6), and a very few minute fragments of the shell.

The mustard seed is, by some, said to contain no starch at all; by others to contain "little or none," and by others to contain "very little," "but a trace," etc. I have not been able to satisfy myself of the presence of starch in the mustard seed, but the cells of the cotyledon are filled with minute granular matter (figs. 4, 5, 6, 7), which almost entirely dissolves in potash with heat, leaving the cells empty with their cell-walls thickened (fig. 8), and in the water in which a dry seed is crushed, can be seen numerous minute drops of oil, similar to what was figured in the article on Capsicum, and many very minute granules (fig. 9), which are solid and clear, and float in the water; they dissolve in potash but do not polarize, nor do they seem to stain by dilute iodine, and they do not swell appreciably on being heated in water. I could discover no trace of a hilum, but am inclined to believe from analogy that these minute granules are, in fact, starch.

In examining samples of ground mustard to detect adulteration, first drop a little into alcohol and see if it instantly gives the yellow color indicative of turmeric; if so, place a little in water under the microscope and determine as nearly as possible the percentage of turmeric present, also note the proportion of wheat-flour, and whether any other starchy matter is present. The addition of dilute iodine will mark the starches, leaving the fragments of mustard uncolored, and thus facilitate the determination; finally add strong potash solution and note what proportion is left after all the starch is dissolved. A fresh sample should also be examined in turpentine, and another should be treated with nitric acid, by which means any fragments of other seeds than mustard will be detected, and lastly, still another sample should be treated with potash and heat, and examined for fragments of seed-husks other than mustard husks. Acid after potash will often soften woody fragments that resisted the potash, and enable their structure to be detected.

1. Dry seed, opaque by ½-in. objective.
2. Shell of dry seed in turpentine; *a*, substance of the shell; *b*, ridges of outer surface; *c*, membrane of outer surface; *d*, inner membrane, + 236.
3. Section of dry seed shell + 215.
4. Section of dry cotyledon + 47.
5. Section of same in water + 215.
6. Separated cells of seed; *a*, of cotyleden; *b*, of radicle, + 236.
7. Fragments of mustard flour + 215.

8. Section of cotyledon cleared in potash + 430.
9. Granules resembling starch, by 1-10-in. homog. imm. objective.
10. Section of soaked seed shell, the ridges swelled up and membrane raised up from the pits of the surface.
11. Cells of outer membrane, in glycerine + 215.
12. Piece of seed shell macerated in acid; *a*, cells of the husk; *b*, ridges of the surface; *c*, cells of outer membrane, distended; *d*, cells of inner membrane; *e*, true membrane of husk cells; *f*, separated blocks of the shell.
13. Appearance of shell and section; *a*, in water, by 1/4-in. objective; *b*, by 1 in.; *c*, macerated in potash; *d*, in acid; *e*, in acid, by 1/4-in objective.—*The American Monthly Microscopical Journal.*

NOTE ON THE SECTIONS OF PINNULARIA.

By M. Prinz.

IN the preceding Bulletin I have presented some remarks on the subject of a note by M. Burgess* relative to the markings of *Coscinodiscus Oculus Iridis* and of *Trinacria regina.* M. Burgess published in the same work some unpublished designs of the late Walker Arnott, representing the structural details of Pinnularia.

From these drawings, the striæ, more or less perpendicular to the raphe, which ornament the valves of this diatom, appear to be tubes.† Their details were studied on fractured edges placed in favourable positions for examination. By inspecting the figures of this work, it is easy to assure ourselves that their natural sections present the same inconveniences as those of the sections of Pleurosigma, obtained by M. Flögel, with which we compare them. They are too thick and often furnish false images, above all with penetrating objectives.

M. Pfitzer, in his admirable work on the Diatoms,‡ has given a very detailed description of the structure of Pinnularia. It is based on the examination of the edges of fractures, and above all, of sections made with the razor by a process similar to that of M. Flögel, that is to say, sections of diatoms embedded in gum. This method does not seem to have given very satisfactory results, as the

* The Microscopical News, March, 1883, p. 71.

† This opinion was also maintained by Schumann (Diatomeen der hohen Tatra, Verhandl, d. Zool, bot. Gesell, Z. Wien, 1867, p. 73.

‡ Pfitzer. Untersuchungen über Bau u. Entrickelung der Bacillariaceen, 1871.

drawings which accompany the work of M. Pfitzer are in part diagrammatic.

I have sought to obtain, by a different process, sections of this diatom, so as to give a more faithful representation. I have chosen a good sample of Franzensbad earth, which our colleague, M. Mauler, has been good enough to procure for me, containing small agglomerations more coherent than the rest of the mass. These were boiled in Canada balsam to harden them. After this treatment they were then ground down by means of emery as other mineral and rock sections.

This method has the inconvenience of necessitating the employment of heat to fix the ground fragment to the glass slip. In performing this operation it is necessary to act with caution and rapidity, to avoid softening the balsam which binds the diatoms

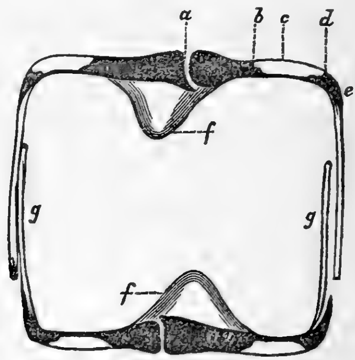


Fig. 14.

together so as not to displace or break them. I have thus obtained thin plates of a centimetre square, containing hundreds of sections of the diatoms perpendicular to the main axis of the frustule. Contrary to my expectations, not a single section gave me a clear image of the raphe. These preparations were always irreproachable and of extreme thinness, in spite of the friability of the substance. Where the raphe and connectives were found the sections had a hazy mutilated appearance, which rendered observations very difficult. I believe I have sometimes seen the thickness of the valve traversed by a broken line, similar to that shown by M. Pfitzer; sometimes this line was more or less oblique.

These appearances are doubtless due to the fact that the balsam, used to consolidate the frustules, does not penetrate into the interior of them: They remain then empty, and their finest details were not sufficiently supported, and so could not resist the grinding

and polishing processes. Such difficulties have probably obliged M. Pfitzer to interpret, by a diagram, the result of his observations on a certain number of sections.

At last I have met with, in one of my preparations, a moderately thick section of an individual of large size. It is perpendicular to the long axis of the diatom, and consequently gives a normal section of the raphe taken around the central nodule, that is to say, in the thickest part, and the more resisting of the frustule as shown in fig. 14. We can apply, point by point, to this figure drawn entirely from nature, the complete and minute description of the structural details of Pinnularia given by M. Pfitzer. I return then to this description, and shall only call attention to the slight differences which exist between my drawing and that of the learned German botanist.

The sections of the raphes present, at this place, the aspect of crevices running perpendicularly towards the interior of the frustule; these divide the frustule, the ends of which terminate in two bends turned in opposite directions.

I have already said that I have not been able to obtain satisfactory sections of this organ in remote parts from the central nodule, where there it appeared placed obliquely.

With M. Pfitzer, I believe that the raphidian crevice inclines more and more in this place, as it raises itself anew towards the terminal nodule. The asymmetry of the central nodules is more apparent; they are also more striking than those which this author has drawn. I have in no case perceived any break of continuity in these nodules (Schumann); they appear entirely solid. The sections of the connectives (hoops), and the slight displacement they have accidentally met with, confirm the opinion of Pfitzer on their mode of attachment, a mode very common in divers kinds of diatoms; but I have not been able to recognise the deep furrow which he says surrounds the connectives. Apart from these questions of detail the similarity is complete.*

To elucidate the nature of the striæ or ribs on the valves of Pinnularia, I have examined sections made parallel to the long axis of the diatom, sectioned by consequence perpendiculary to the ribs which furrow the valves. These longitudinal sections are present in great number in the thin plates prepared from the earth of Franzensbad, rarely they are defective. When one of these sections is too thick it comprises a greater or less portion of the rephidian

* M. Flügel has equally studied sections of Pinnularia, but the conclusions are only known by a very short resumé which appeared in the Botanische Zeitung, 1872, p. 741. This able micrographer there describes some types of diatoms in section. The Pinnularia is shown in one section, and it is added :—" A large empty space which has only one passage in the form of a short canal towards the interior."

region, or a part of the edge of the valve. Sections of a thickness equal to *ca*, *ce*, *ad*, *be*, fig. 14 will illustrate this case. If we are looking at a section having a thickness *ca*, for example, with a low-angle penetrating objective, the ribs appear invariably closed at the upper part by a membrane, in a word, it appears to resemble a section of cylinders. This image is produced by the objective showing at one and the same time the section of the ribs, and the lines representing those parts of the valve much more deeply situated.* The result is an image difficult to interpret in any other way than by the presence of cylinders. (Fig. 15).

Fig. 15 is the view given with Zeiss' dry $\frac{1}{14}$. Fig. 16 represents the same section under Tolles' $\frac{1}{10}$ homog. immersion. Fig. 17 does not alter whatever objective may be employed. But if an objective without penetration is employed on a very thick section, the planes lie at such distances apart as to prevent the various images confounding each other, and so this cause of error is eliminated. (Fig. 16).

We have considered up to now, sections of considerable thickness. In those which are thinner, the edges of the valve, and the

Fig. 15.

Fig. 16.

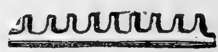


Fig. 17.

solid parts which run on the raphe are ground away, and there remains only the section of the ribs themselves *b d*, fig. 14.

An image is obtained as shown in fig. 17 which leaves no doubt of the presence of parallel elevations separated by spaces a little wider than their thickness.

I need not insist on the utility of sections for the study of diatoms, it is by this means alone that we may expect to have exact notions of their structure. All the methods of sectioning are not equally good. Sections prepared by embedding the frustule in gum often give rise to false interpretation, the centres are not entirely solid, and so many of the more delicate parts are ground away and disappear. Suppose for a moment that the raphe to be only a very deep channel, but closed towards the interior (as other observers admit)† by a fine membrane, the least violence will suffice to rupture the section at this spot, and to give the image of a crevisse traversing the thickness of the valve right through. This effect cannot be produced by the process before described, because

* The drawing of Walker Arnott, reproduced by M. Burgess (*loc. cit* fig. 27), represents a fragment of a valve having a thickness equal to *b e* of fig. 14. Perhaps it comprises the total thickness of the semi-valve.

† Schmidt. *Botanische Zeitung*, 1872, p. 741, *et. seg.*

the parts are maintained by the solid parts of the valve situated farther away, and above all by the hardened balsam which surrounds them. A cement absolutely solid does not offer these inconveniences. Balsam is only useful to give a soft rock consistency and a greater hardness, it cannot be employed to agglutinate diatoms already in powder. Other cements I have used have given me nearly negative results, but I may mention one method which seems to possess advantages, viz., the employment as a bedding agent of the solid matter deposited by certain incrustating waters. The water from these springs is often employed to obtain copies of medals, bas-reliefs, &c., and on being left to itself deposits a hard matter consisting in great part of carbonate of lime. On mixing frustules of diatoms with these mineral waters a hard deposit may be obtained, from which it is easy to cut very thin and perfectly transparent sections. Further than this, the calcareous cement may be easily eliminated by weak acid, thus leaving the isolated sections of the diatoms in a state so that they may be mounted separately in a medium more favourable to their study.

It is not absolutely necessary to have recourse to the artificial methods of preparation. Many sufficiently hard rocks contain diatoms in greater or less quantity. Certain varieties of guano are hard and yield very good sections. There is there for you a virgin soil which will furnish quite as much interesting observation as you may wish to explore.—*Bull. Soc. Belge de Microscopie, June 30th, 1883.*

THE OVARY OF A POPPY.

SO far as the mere preservation of the individual life of any particular plant is concerned, the only physiological duties required to be performed by the organism are those every-day operations connected with its respiration and nutrition; but as it is necessary at an earlier or later period of life to make some provision for the preservation of the species, a special physiological function at this time manifests itself, resulting, under favourable conditions, in the production of offspring, which, strongly inheriting the structural characteristics, modes of growth, constitutional peculiarities, and so forth of the parent, carry forward to another generation the direct line of specific descent. In the higher plants, at all events, this all-important provision is effected by the formation of *embryos* contained within structures known as the seeds, each embryo being the direct outcome of a sexual act; that is to say, it is produced, or rather its developmental growth is initiated

by the coalescence of two masses of protoplasm differing very much from one another physiologically, and derived either from different parts of the same plant or from, what is perhaps more usual, a different plant altogether.

The germ or female element concerned in the process, is a stationary, nucleated mass of naked protoplasm, lying within a comparatively large cell or cavity called the *embryo-sac*, which is a specially developed sub-epidermal cell of the *ovule*, or incipient seed; while the sperm or male elements are developed, generally in great numbers, within usually large sacs, or pouches, known as the *anther* cells. Each mass is small, usually more or less rounded, and becomes early invested with a cellulose cell-wall, that eventually differentiates itself into two layers, the inner remaining excessively thin and unmodified, while the outer gradually resolves itself into a surface of cork, specially adapted to act as a protection to the sperm protoplasm, which, sooner or later, is destined to become detached, and to lead, under favourable conditions, a short yet independent existence, removed from the structure within which it was generated. For it should be known, these sperm masses, or *pollen grains*, as they are called, have this special physiological function to perform in the reproductive economy of the plant; they have to carry to the germ mass the necessary protoplasmic material, or highly specialised sexual food, which, by a process of transfusion, commingles with the female element, and enables it to start a process of growth, and series of developments, which end in the formation of an embryo. After this duty is performed, the activity of the pollen ceases, and it quickly dies.

But in order to understand the exact mode by which the sexual act is affected, let us study as a type the poppy plant, and therein observe how the germ mass receives the fertilising contents produced within the sperm or pollen grains. The ovules or organs containing the germ mass are themselves contained in special vessels the *ovaries*. The ovary is the youngest or central structure of the flower, and in the poppy it is large, oblong or subglobular, surmounted by a conspicuous discoid cap, composed of a number of radiating and cohering *stigmas*, which have surfaces specially developed for the reception of the pollen grains, and which also, at a certain period, secrete a stimulating fluid that tends to encourage the growth of the sperm protoplasm.

Now in plants, like the pea, for example, we have an ovary of a much simpler nature than we find in our type. There, as every one knows, we have an elongated ovary which, during the formation of the seeds, or peas, ripens into a pod. The ovary has a single cavity, and may be looked upon as a leaf which has undergone a special development, whereby its opposite lateral margins have become inflexed and cohering, while from the sutural line of

cohesion ovular outgrowths arise at intervals along its entire length. Such a leaf, producing in this way female organs of reproduction, is called a carpel, and the ovule-bearing region is known as the *placenta*. The ovule is usually borne on a stalk—the *funicle*—which is the connecting cord between the parent and its egg, and serves as a channel for the conveyance of food and air to the young embryo during its growth within the embyro-sac.

In tulips and lilies the ovaries are compound structures composed of three such carpels as we find in the pea, but instead of each carpel growing separate from its neighbour, as seen in our larkspurs and anemones, they cohere along their sides, forming a structure which, when cut across, shows that it has an internal arrangement of three cavities, or "cells," with the ovules growing to the acute inner angle of each cavity.

But we may have an ovary where the carpellary leaves are united after a different fashion. In violets and in the mignonette, for example, the ovaries are composed (usually) of three carpels each, yet, as may be easily seen, the structure is only one-celled. Here, however, it is evident that each carpellary leaf has not been folded in the ordinary manner, so as to form a separate cavity for its own ovules prior to the cohesion of the carpels among themselves, as was found in the lilies, but that the adjacent margins of neighbouring and but slightly curved or open carpellary leaves have become united, so as to form one large cavity common to all the carpels; and that, therefore, each of the three somewhat swollen placentæ seen on making a transverse section of the ovary of a pansy is formed by the cohering margins of two similar but individually distinct, carpellary leaves.

In the poppy we have also a multi-carpellary ovary and one, too, where the arrangement of the carpels follows the same general plan found prevailing in the violets and mignonette. The carpels produced in each flower are, however, more numerous—ten being a common number—and each being just slightly curved and the whole ten being arranged circularly with the opposed sides or margins of neighbouring leaves cohering, as in the violets, we have, as a structural result, the formation of the characteristic oblong or sub-globular ovary of our type. The placentæ, which, as we have seen, are just slightly swollen in the mignonette, are in the poppy developed to an enormous extent. They stretch themselves out into the cavity of the ovary almost, if not quite, to the centre, forming a circle of vertical plates from the broad surfaces, of which the numerous ovules spring, so that in a thin transverse section of the whole organ, as has been made in the accompanying preparation, there is clearly displayed, even to the unaided eye, the outer, or ovary wall of circular outline, with a number (ten or thereabouts) of radiating ovule-bearing bands, each attached by one end to the

inner side of the ovary wall, while the other is lying free at the centre, or in the vicinity of the centre of the section. By first examining the section with an ordinary lens, then with a low power of the microscope, and finally a representative portion of the structure, with such a power as that under which the accompanying drawing was made,* a comprehensive knowledge of the whole will be gained with a little study. Imbedded in the ovary wall, and opposite the origin of each placenta will be seen a section of a fibrovascular tissue cord, which has been cut across. These twigs carry upwards the nourishing sap to the growing ovules; the cords being in direct structural connection below with a similar system, which ramifies throughout the entire poppy plant. The placentæ are principally made up of a rather loose cellular tissue, through which, however, run tiny lateral offshoots from the parietal fibro-vascular cord, and which themselves send off laterally into the funicle of each ovule, a very fine twig to conveniently supply, as beforesaid, the growing embryos with assimilated sap.

The section represents the ovary at a time prior to that of the fertilisation of the ovules, but before describing the act of fertilisation, it will be necessary to learn that the ovule proper is invested in a double coat, everywhere continuous excepting over a spot which is opposite to the sub-epidermal embryo-sac; here an inconspicuous opening is left, which, in botanical terminology, is known as the *micropyle*. Then, when the female element is ready to be fertilised, the stigma "ripens," that is, it fully extends itself, becomes sticky, or otherwise prepares itself for the reception of the free-moving male element, or pollen. After the pollen reaches the stigma—and the conveying agent may be the wind, insects, or simply gravitation—it immediately begins to grow by sending out a tube, bounded by the extended inner covering which, on first lengthening, bursts through the outer and corky coat of the grain. The growing tube pushes itself into the substance of the stigma, and continuing its growth downwards, enters the cavity of the ovary. Here it finds an ovule, and, entering it by way of the micropyle, the end of the tube carrying the sperm substance soon approaches the germ mass and fertilises it. After this act the fertilised mass begins to grow, and, fed by its parent, eventually develops itself into a new individual, while, at the same time, the ovule is being gradually transformed into a seed, and the ovary into a fruit.

The constitutional vigour of the seedling, its own inherent reproductive powers to be afterwards displayed, the number of seeds produced, and such-like important matters bearing upon the plant's vitality, depend largely upon the comparative relationships

* A beautiful chromo lithograph illustrates the original article.

that exist between the fertilising masses. For example, the relationship must not be so distant that the sexual elements are derived from sources outside the same natural group; while, on the other hand, it has been proved, over and over again, that, in the majority of cases, at all events, self-fertilisation—that is, the transportation of pollen from the anther of one flower to the stigma of the same flower—does not bring about the best progeneratery results. Perfection of the highest order can only be attained when the ovules have been cross-fertilised—that is, when the stigma of one flower is fertilised by pollen derived from the anther of another flower.

Now the form, arrangement, and number of parts, periods of ripening of anthers and stigma, colouring, time of flowering, hours of opening, and other matters in connection with the matured flower, have all more or less important bearings upon the act of fertilisation, and in the majority of plants, tend to obstruct or entirely prevent self-fertilisation, and encourage the more beneficial reproductory act of crossing.

If the fertilisation is to be effected by means of insects, then the flowers have generally showy, easily-seen, and attractive petals, almost always sweet or otherwise scented, while they often also develop nectaries, so placed that to reach them the winged visitors must brush past, push themselves against or walk over some floral organ or organs, so that while they are contentedly sipping the honey, the pollen is being either collected by some part of their hairy bodies, or else being removed from their dust-covered bodies by the sticky surface of a stigma.

On the other hand, if the flowers are wind-fertilised, then the flowers are small, green, and inconspicuous, absolutely scentless, and certainly without honey; and instead of the stigma being more or less knob-like and sticky, it is often either cut up into a tassel-like structure, or else densely hairy, so as to increase the chances of collecting the wind-borne pollen. Compare the showy flowers of the poppy, fuchsia, and honey-suckle, for example, with the inconspicuous flowers of oak, hazel, willow, or oats; while the broad and platform-like, and consolidated stigma of poppy, as compared with the two, long, slender, densely hirsute diverging stigmas of oats, clearly show that these organs are specially adapted for the performance of somewhat different functions, one in fact for insect and the other for wind fertilisation. In the poppy itself each flower produces a great many anthers, and these, borne on rather long filaments, stand erect and close around the radiating stigma. The petals are very large, and, being brightly coloured, are hence very attractive to insects. As the flowers do not secrete honey, they are, therefore, only visited by pollen-seeking insects, which find the broad stigma a most convenient landing stage. The anthers, more-

over, burst before the stigma ripens, and the pollen is thus liable to be, at least in greater part, removed before the stigma arrives at its full maturity, leaving it, therefore, greatly dependent for its full complement of pollen upon supplies brought to it by the pollen-dusted bodies of insect visitors. It is likely, however, that many of the extremely numerous ovules may be fertilised by pollen derived from their own flowers, that is self-fertilised. After fertilisation is effected, and embryos are being formed within the embryo-sac, simultaneous changes of a profound nature are also taking place in the structure of the whole ovary. While the now useless petals and stamens are dying the two coats of the enlarging ovule are getting firmer and tougher, the outer becoming more or less coarse, and well suited to act as a protection to the rapidly forming seed. At the same time the ovary walls enlarge, get, perhaps, somewhat stouter, and lastly drier, then when the contained seeds are able to live independently, and become detached from the dried up placenta plates, little valves open at the upper end of the seed vessel, immediately below the eve of the persistent stigma and from the pores or openings so formed, the small, almost round, or kidney-shaped, pitted seeds can easily escape at each bending of the stem in the early autumn breeze.

At another time we will attempt a closer study of the anatomy of a seed, and trace the early independent growth of the embryo when thus detached, and removed from the parent, together with the more important physiological facts connected with the phenomenon of germination.

Enough, however, has been said, at present, to explain, in a general way, the structure and use of the flower and to broadly indicate how wind and insects may aid the plant in the dissemination of its pollen, enabling it to produce many and healthy seeds, and especially how insects, following their instinctive tastes for bright colours, strong scents, and sweet fluids, are unconsciously developing in plants a diversity of floral forms, encouraging a brilliancy of colouring and increasing their scent-producing and honey-secreting capacity ; and that, therefore, the visits of insects have a decided and very important influence upon the plant's reproductive results, and those flowers that are the greatest favourites with the winged hosts of bees, flies, butterflies, and moths will be the most prolific, and will be the progenitors of new individuals which, inheriting the habits and structural peculiarities of their parents will, still further, perhaps, in their turn, develop those characters which influence the destiny of their race.

Method of Preparation.

The ovaries from which the accompanying preparations were cut were obtained from the just opening poppy flowers gathered in a cornfield at Reading in the month of June last.

After separation from the other organs of the flower they were placed in common methylated spirits for twelve hours, after the lapse of which time they were removed, embedded in carrot and cut with a sharp razor. A hole of sufficient size may, by the way, be easily cut out in the carrot with the barrel of an ordinary pen.

After cutting, the sections were placed in distilled water for a few minutes, in order to soak out the spirit.

From this, they were conveyed into the staining fluid prepared by adding five drops of Martindale's logwood solution to one ounce of distilled water, and allowed to remain there for about one hour; they were then removed, washed in distilled water, and placed in rectified spirits for two or three hours to remove the water, after which they were taken out and floated on the oil of cloves contained in a large watch glass. So soon as they were observed to sink they were removed and mounted in Canada balsam.

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The following very limited list of Works is given in the hope that it may be found useful to Students commencing the Study of Botany.

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A LANCASHIRE NATURALIST IN NEW SOUTH WALES.

ABOUT twelve months ago we recorded the departure from Ashton-under-Lyne of Mr. Thomas Whitelegge, one of the hardworking group of artisan naturalists of whom Lancashire is justly proud. A letter recently received from him by friends here

contains, in addition to pleasant personal gossip, some interesting notes on the flora of Australia. Mr. Whitelegge is now a member of the Linnæan and Royal Societies of New South Wales, and is at present temporarily engaged in the mounting of a large collection of shells at the Australian Museum in Sydney. When this task is completed he will probably go to Western Australia on a collecting expedition for the Hon. W. Macleay, who has a very large private museum. The following extracts will be read with intererst:—"I have done very little collecting as yet, although I am always out somewhere. The number of British plants naturalised here is astonishing. As you walk along the roads or cross the fields you might almost fancy yourself at home. *Fumaria officinalis*, *Lepidium sativum*, *Sisymbrium officinale*, *S. didyma*, *Capsella bursâpastoris*, *Erodium cicutarium*, and *E. moschatum*, *Malva rotundifolia*, *M. sylvestris*, *Urtica urens*, *Stellaria media*, *Cerastium vulgatum*, and so on, besides nearly all the English docks, thistles, and chenopodiums, with trifolium, melilotis, &c. Plantains, and plants belonging the order Compositæ, and grasses are everywhere. As to the Australian plants, I can say but little, as I know only a few. Out in the bush there are plants in abundance, banksias, eucalyptas, mellilucas, and acacias being the most abundant; also a vast number of leguminous plants. Orchids are plentiful, and very pretty. Sundews literally cover the ground in damp situations. *Drosera spathulata*, *D. dichotoma*, *D. peltata*, are found close to where I live, which is but a few minutes' walk from a big swamp some eight or ten miles in extent, full of sedges and liliaceous plants. There are many pools and streams. There are nitella, chara, mosses, sphagnums, and liverworts in plenty; in fact the swamp literally swarms with life: snakes, frogs, and lizards move about in all directions as you walk along. I have found many things that had not been observed here before. Some of these are quite new. So far, I have found three species of fresh water polyzoa —namely, *Plumatella repens*, *Fredericella Sultana*, and one not identified; about ten or twelve species of tube-building rotifers— *Melicerta ringens*, *Œcistes janus*, *Œ. pilula*, *Limnias ceratophylli*, *L. annulatus*, *Cephalosiphon limnias*, and two new species. Of *Floscularias* I have found about six species, two of which I believe are new. I have also found two species of sponges, one new to science, and the other only found in Queensland. I have added to the lists of plants known in the Sydney district chiefly nitellas, &c. Ferns are very pretty here, but I have not paid much attention to them yet. I have seen *Hymenophyllum Tunbridgense* in plenty, and *Adiantum hispidulum*, *A. affine*, *Gleichenia circinata*, and many others. The *Azolla pinnata* covers the water here like duckweed at home. It is really a sight worth seeing, the plant being of a dark brownish-red colour. I have not yet seen any

spores produced. Volvox is very plentiful. I have lately collected a few shells, being within easy distance from the seaside, or rather quite close to the harbour, for on the shore of the open sea it is not safe to venture on the rocks as there is such a tremendous swell. This is always on. Even when the sea is smooth the swell is simply frightful. You may get down on the rocks and think you are all right, when the sea comes rushing in two or three yards high, and you have to run at once, or be in danger of getting washed out into deep water. Many a time I have turned over a stone and seen a nice lot of shells, but before I could pick them up I have had to run, and of course the shells are washed away. There are no boats on the coast on account of the surf and sharks, which are numerous. In fact bathing is only safe in places fenced off for the purpose. I got a fine lot of *Spirula*, a shell belonging to a cuttle fish, being the only living representative with an internal chambered shell and representing some of the fossil shells not very far from the belemnites. There is a place about ten miles from here where the *Echidna hystrix* is found, but it would require ferrets to get them, or I should have made an attempt before now."

PRACTICAL PROCESSES IN VEGETABLE HISTOLOGY.

By L. Oliver, in Rev. Sci. Nat., 1882.

(Continued from page 4.)

IV. Precipitation, Crystallization.

THE substances whose precipitation or crystallization is produced in the interior of the cells are asparagin, inulin, and the saccharoses. Their deposition can be incited by a solution which contains principles different from those that are being sought for or even (according to the method originated by Borodin *) saturated with the substance itself which it is proposed to discover.

Asparagin.—Asparagin crystallizes in this way in cells when treated with a saturated solution of asparagin. It is even the best means of showing its presence. It is obtained in greater quantity by immersing the tissues in absolute alcohol, which on subsequent evaporation leaves the asparagin in crystals. But as the alcohol also takes up other substances capable of crystallizing, in order to

* Bot. Zig., 1878, p. 804.

recognize it, we treat all the crystals with a concentrated solution of asparagin, in which this substance alone remains crystallized.

It should be observed that the tissue in which it is to be studied ought to be in active life, since asparagin, which is an acid of bimalate of ammonia, constitutes a product of secretion, as it were the urea of plants.

Inulin.—Solid inulin can be obtained in the cells in two different conditions; in the amorphous or the crystalline. Desiccation causes the precipitation of this substance, which previously existed dissolved in the cell-sap; it is most frequently amorphous. Nevertheless, when desiccation is very slow it crystallizes.

Prolonged maceration of the organs which contain the reserve-materials in alcohol causes the formation of sphero-crystals of inulin. When sections are made of the tissue thus prepared a little acetic acid is added, and they are put in glycerin.

The alcohol used must be diluted with water. It is advantageous to reduce imperceptibly, by evaporation, the quantity of water added to the alcohol, and to keep up the level of the liquid in the vessel by adding to it gradually absolute alcohol.

When there is not time to allow the organs to remain in the alcohol before making sections, the sections themselves can be subjected to the action of either absolute alcohol or ether. In this case a deposit of amorphous inulin is obtained.

Saccharose.—The saccharoses are insoluble in absolute alcohol. It is, therefore, sufficient to treat the saccharine cells by this agent in order to produce the crystallization of the saccharose. Bonnier* has often had recourse to this process in the examination he has made of the nectaries. By way of verification he treated the soluble portion of the tissue with 80 per cent. alcohol and with ether; he then saw crystals of the same form appear in the liquid.

Sections made transversely to the saccharine tissues can also be allowed to dry. In evaporating, the cell-sap leaves the saccharoses in the form of stellate crystals, the crystallographic system of which it then becomes possible to recognize.

Aleurone.—This is the place to point out the means of preserving from solution in water the proteid part of the aleurone grains. It is known that in several plants, the peony for instance, this portion of the grain is very soluble in water. It is rendered insoluble by first subjecting it to the action of an alcoholic solution of bichloride of mercury. It is on this very phenomenon that Pfeffer relies to establish the presence of a quarternary nitrogenous substance in the aleurone grain.†

(To be continued.)

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* "Les Nectaires," Ann. Sci. Nat., 1879.

† Pfeffer, Jahrb. f. Wiss. Botanik., viii. (1872).

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Tubbs, Brook, and Chrystal, 11, Market-street, Manchester, to whom also all applications for advertisements should be made.

ALL matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, The Willows, Fallowfield, Manchester.

THE JOURNAL OF MICROSCOPY.—The Journal of the Postal Microscopical Society appears this year under the above title. We commend it to the notice of our readers.

ADDRESSES WANTED.—Can any of your readers oblige me with the addresses of:—

1. The Scottish Naturalist.
2. The Yorkshire Naturalist.
3. The Northumberland and Durham Natural History Club Hon. Sec.

W. B. Grove, The Mason College, Birmingham.

THE FAIRY FLY.—Mr. Frederick Enock has sent us a very beautiful preparation of the fairy fly, which, as many of our readers will know, lays its eggs within those of butterflies. Sometimes as many as three or four of the tiny larvæ exist in company in one single butterfly's egg, where they change to pupæ, the perfect fly eating its way out at last. It is shown to perfection under the one-inch objective and illuminated with the spot lens.

PUBLIC AQUARIA.—We are sorry to notice that our printers neglected to acknowledge the source of the article upon this subject which appeared in our last issue. It was taken from the pages of the American Monthly Microscopical Journal.

THE MIDLAND NATURALIST.—This Journal is now entering into its seventh year, and its circulation is about to be extended by the supply of each number to the members of the Birmingham Natural History and Microscopical Society. In order to enable this to be done, it is proposed to raise the subscription of members to one guinea per annum.

COLE'S STUDIES.—Several numbers of these important "studies" have appeared since we went to press with our last number. We have selected one of these for publication in order to show our readers the excellent manner in which Mr. Cole does his work. The article selected has been "The Ovary of the Poppy," but the

article alone gives but a poor idea of the whole work, as the original is illustrated with an excellent chromo-lithograph and a perfect slide, to illustrate the subject.

COLE'S STUDIES.—Numbers 7 and 8 of Volume II. are before us; the former contains an account of epithelial tissue, simple, stratified and transitional, the simple being tabulated as pavement, columnar, glandular and ciliated. A coloured plate illustrates the slide accompanying the number, demonstrating the structure of epithelium from the tongue, intestine, and fauces.

No. 8 treats of the cell as an individual, and is described and illustrated by a beautiful plate of the desmid *Micrasterias denticulata.*

No. III. of the Popular Microscopical Studies with its accompanying slide is devoted to the Hair follicles, glands and tissues of the Human scalp. The fifth part of "The Methods of Microscopical Research," is a continuation of the preparation of tissues.

STAINING AND MOUNTING POLLEN.—The following appeared in the American Monthly Microscopical Journal for 1880, as the process of preparation adopted by the Rev. J. S. Brownell, who exhibited, at a meeting of the New York Microscopical Society, some slides of stained Pollen of special excellence.

"A small quantity of Pollen having been placed on the centre of the slide, a small drop of staining fluid (anilin dissolved in alcohol), is placed upon it. Then wash by dropping on pure alcohol until all traces of sediment or of stains upon the glass among the Pollen grains are washed away. Wipe clean with a dry cloth drawn over the end of a pointed stick, turning the slide rapidly on the turn-table. When thus cleaned and quite dry, put on a drop of spirit of turpentine, and then the balsam and cover."

"A few kinds of Pollen are distorted by the action of alcohol. Some of these can be stained by the use of an ammoniated solution of anilin. Those that will not bear this solution may be mounted unstained."

PHOTOMICROGRAPHY.—I have often thought if Microscopists knew how easy it was to adapt an ordinary photographic camera for Microscopical work they would employ photography much more than they do at present. For any one intending to read a paper before any scientific society it is very useful to have it illustrated in some way, and as a rule Microscopists are not good at drawing, and further find the camera lucida a tedious and not very satisfactory process. No doubt many have thought photography would be a very convenient way, but that it would entail a special camera. Such however is not necessarily the case, and using an ordinary camera has the double advantage that it can be used for ordinary photography when not required for the microscope. The difficulty

is, therefore, got over of either having to keep two cameras, or have a special one. In using the ordinary camera it is necessary to have it raised high enough for the tube of the microscope just to enter the hole where the lens is screwed in. Any one handy with his fingers can soon make a stand for it to be fixed to when in use. Of course the microscope has to be brought to the horizontal position, and it is necessary to keep the eye piece in its usual place. I have found the Kelner eye piece the best for the purpose, as it gives a good field.—JOHN E. FAWCETT.

NOTTINGHAM NATURALISTS' SOCIETY.—October 16th, Mr. G. Mundon read a paper on "Tokens," which was full of interesting facts about the various kinds that were issued chiefly during the reign of George III. He also exhibited a number of tokens with their attendant forgeries, the more remarkable among them being Bank of England 3s. tokens, with their forgeries, a 1s. 6d. (English), and a 1s. 6d., a 10d., and a 5d. (Irish), with a number of light private tokens, 1s., 6d., and other forgeries. Nov. 6th, Mr. Henry Blandy, L.D.S., read an interesting and instructive paper on "Some points of interest in the Comparative Anatomy of Teeth," which was illustrated with diagrams, microscopic slides, and specimens. Nov. 20th, Mr. Councillor Hugh Browne read a paper on "What is the meaning of Vegetable Life?" which led to a long and spirited discussion. Dec. 4th, Mr. B. S. Dodd, Hon. Sec., read a paper on "Savoury Dishes (animal and vegetable) not usually eaten," in illustration of a series of dishes, one of which he had had prepared for each meeting of the Society for the past few weeks. The series comprised (1) roast hedgehog; (2) sea-weed jelly and blanc mange; (3) fricassee of frogs (French); (4) rat pie (English barn rats); (5) French snails (from Paris); (6) Iceland moss jelly. The remainder of the evening was devoted to the examination of fresh-water pond life under microscopes.

PREPARATIONS OF COAL.—P. F. Reinsch's preparations of coal from the carboniferous strata, the Dyas and Trias (the material being very difficult to reduce to thin and sufficiently transparent sections), are made by using the finest emery employed in polishing mirrors; powdered chalk obtained by levigation, and carbonate of lime precipitated from lime-water by soda are also used. A small piece of cork serves as a rubber. During the process the preparation is moistened with glycerine.—*Bull. Soc. Belg. Micr., IX., p.p. 87-8 (1883.)*

SAFETY STAGE FOR THE MICROSCOPE.—At the meeting of the Royal Microscopical Society on November 14th, Mr. Stewart exhibited a safety stage which he had invented, chiefly to meet the want which is sometimes felt in exhibiting a perhaps valuable slide to a class of students, or other inexperienced persons, who are very

apt to break the cover-glass by racking the objective down upon it. A piece of wood rather wider than an ordinary glass slide has a hole cut in the centre large enough to admit the light to the object. Between this hole and the sides of the piece of wood two small strips of wood are fixed, and on the top of each of these is a thin strip of brass, rather longer than the strip of wood, so as to overhang at each end. A couple of india-rubber rings are then passed, one round each pair of projecting ends, and between these, suspended in a kind of hammock, is placed the slide which it is desired to protect. If then the objective is brought down upon the cover-glass, the india-rubber springs yield to the pressure, and the object is saved from destruction.

THE NEW BARONET.—Mr. Joseph Lister, F.R.S., LL.D., of Park Crescent, Marylebone, one of the Surgeons Extraordinary to Her Majesty, upon whom a patent of baronetcy has been conferred on account of his professional ability and services, is an M.B. of the University of London (1852), a Fellow of the Royal College of Surgeons, England (1852), and a Fellow of the Royal College of Surgeons, Edinburgh, (1855). He was for some time Regius Professor of Surgery in the University of Glasgow, and Assistant Surgeon and Lecturer on Surgery at the Royal Infirmary, Edinburgh. In 1876 he was one of the members appointed for Scotland by the Privy Council to the General Medical Council. In 1880 he received the medal of the Royal Society, and in the following year the prize of the Academy of Paris was awarded to him for his observations and discoveries in the application of the antiseptic treatment in surgery, which has often been referred to as "Listerism." He received the degree of LL.D. at Glasgow University in 1879, D.C.L. at Oxford in 1880, and LL.D. at Cambridge in 1880. Sir Joseph Lister, according to the "Medical Directory," is the author of papers "On the Early Stages of Inflammation," &c., in the "Philosophical Transactions;" "On the Minute Structure of Involuntary Muscular Fibre," in the "Transactions of the Royal Society of Edinburgh;" "On the Muscular Tissue of the Skin," in the "Microscopical Journal," and of various other papers on "Surgical Pathology."

THE APPLICATIONS OF SECTION CUTTING.—In the Lord Mayor's Court, recently, before the Recorder and a common jury, the case of *Ricardo* v. *Abrahams* was heard. This was an action brought by the plaintiff, a dealer in precious stones, at 236, Southgate Road, against the defendant, a jeweller, of Houndsditch, to recover the sum of £17 14s. 4d. for a parcel of imitation sapphires sold and delivered. The defendant pleaded a denial of liability, alleging that the supposed stones were sold to him as real and turned out to be imitation. Mr. Innis was counsel for the plaintiff; Mr.

Geoghegan for the defendant. The plaintiff's case was that on the 20th of September last he met the defendant in the street and offered him a pacel of imitation sapphires, known as "beryl sapphires." Mr. Abrahams referred him to his son, who was at home, and that gentleman purchased the parcel for the sum now sued for, having till the next morning to decide upon the matter. He had, however, subsequently refused to pay for them, alleging that they were not, as represented, "inferior sapphires." The defendant's son was called, and said the plaintiff represented at the time he brought the parcel to him that the contents were real stones, but of an inferior quality. He sold some of them at a small profit to his brother-in-law, a jeweller, who gave him a cheque, which he stopped two hours later, informing him that the supposed stones were nothing but "paste," and worthless. He thereupon refused to complete the bargain for the purchase. Mr. Alfonso Nourick, a lapidary, carrying on business at Upper Gloucester-street, W., said he had examined the supposed stones. They were of the commonest kind of imitation known as "paste," and sold by the gross. Mr. Geoghegan : What does "paste" mean? Witness: A mixture of lead glass, and borax. (Laughter). Mr. Geoghegan : And what are imitations? Witness: I make imitations, but I make them out of real stones. Mr. Innis : How can that be? Witness: I will tell you; perhaps I take some pale stones which would not be valuable. I split those, introduce the colour desired, and then join them again. (Laughter). By that means the public get real stones at a cheap rate, representing stones of a greater value. (Renewed laughter). I never heard of a "beryl sapphire." "Beryl" is a valueless crystal of various colours, but would mean "real," and therefore I should consider a "beryl sapphire" to mean a "real" stone. Another witness, Mr. William Jennings, was called, who examined the supposed stones and pronounced them to be paste. Mr. Innis : Are they well got up? Witness: Yes. Mr. Innis : They would take anybody in, would they not? Witness: That is exactly what they were intended to do, I should think. (Loud laughter). Ultimately the jury found a verdict for the defendant.

MANCHESTER MICROSCOPICAL SOCIETY.—At the last meeting of the Mounting Section of the Manchester Microscopical Society, there was a large attendance, Mr. J. L. W. Miles presiding.

The Chairman, referring to the use of Canada balsam as a mounting medium, said that, broadly speaking, there were two ways of viewing microscopic objects; by reflected light when looking at external surfaces, and by transmitted light when investigating internal structure. Canada balsam has the property of rendering most objects, after certain treatment, very transparent, and for this reason was largely made use of. Other media, such

as Farrant's solution, glycerine, or jelly, were sometimes preferable where tissues were of a very hyaline nature, but with objects requiring a maximum amount of light for their illumination, especially for use with the polariscope, balsam was, from its highly refractive nature, very suitable. Mr. Miles afterwards mounted successfully several specimens of the head of the honey bee in pure balsam without pressure, a method of mounting advocated by many, but not generally adopted, in consequence of manipulative difficulties. Increased interest has recently been excited by some beautiful entomological preparations thus mounted and sold by Mr. Frederick Enock, of London. These mounts command a comparatively high price; hence the ability to prepare objects in this manner is worth acquiring by the student in microscopy. Apart from their value as show objects, these mounts are almost indispensable as aids to a correct knowledge of the disposition of parts—the relation which one organ bears to another—a knowledge of which should always precede the investigation of a particular organ with high powers. Two difficulties are usually met with in the putting up of this kind of mount—imprisonment of air bubles and shrinkingage of the medium by evaporation—to obviate which, in the use of balsam, a new cell, having alternate elevations and depressions, has been devised by a member of the section, in the use of which, by leaving an excess of balsam round the cell and cover glass, air bubbles ultimately escape through the spaces, and loss by evaporation of essential oil in the balsam is provided for. Mr. Miles incidentally remarked that the preparations of Mr. Enock were, he believed, put up in glycerine, a medium he would make use of some evening when fluid mounts were under consideration.

Mr. E. Ward illustrated mounting in balsam and benzole. The addition of benzole to balsam renders it more fluid and less difficult to work with, but in selecting Polycystina and Spicules to work upon Mr. Ward had to face difficulties which, under his skilful manipulation, were not apparent. Few microscopists have the opportunity of seeing Polycystina alive, and besporting themselves in their native element, the sea, but nearly all are familiar with the beautiful siliceous skeletons of these minute creatures. They are found plentifully in a fossil state in Bermuda, Sicily, Virginia, and Barbadoes. They are of wonderful beauty and variety of form; are more or less perforated and covered with spines and projections, through which the sarcode body when alive extends itself into pseudopodial prolongations. When seen besporting themselves in all their living splendour, their brilliancy of colouring renders them objects of unusual attraction. They are met with on the surface of the Atlantic, Pacific, and other Oceans, and were obtained in Mid-Pacific at 2,425 fathoms during operations by the Challenger ten years ago.

THE MICROSCOPICAL NEWS

AND

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LIVERPOOL MICROSCOPICAL SOCIETY.

ADDRESS OF THE PRESIDENT, CHARLES BOTTERILL, ESQ.

DELIVERED JAN. 18TH, 1884.

AMONG the many revelations of the microscope there is one which, to my mind, is of surpassing interest, and that is the existence of that curious substance—protoplasm. Not that this can compare for beauty with thousands of other objects which the microscope makes us acquainted with; for even an amœba can scarcely be said to have many personal attractions but because of its intimate connection with life, and the wonderful powers with which that connection endows it. Huxley has styled it the Physical Basis of Life, and Allman has well said of it—"Wherever there is life, from its lowest to its highest manifestation, there is protoplasm; wherever there is protoplasm, there too is life," which of course implies that protoplasm is the only substance possessed of life, and (as a corollary) that where there is no life there is no protoplasm. Being then so intimately connected with all living things it ought to be of the deepest interest to us, but I fear that amongst the many more attractive objects revealed by the microscope it is apt to be overlooked. I propose, therefore, to call your attention this evening to this singular substance, confining myself, however, to a few of its most striking powers, and the inference which may fairly be deduced from them; as even if I were competent to treat the question fully, the time I have allotted to myself would not suffice.

In the first place, however, let us devote a few moments to the consideration of what protoplasm is. As it is generally described, and as we usually see and think of it, it is a semi-fluid substance—a tenacious, glairy fluid, not unlike the white of an unboiled egg, which in chemical composition it closely resembles—homogeneous, indifferentiated, and totally devoid of structure. This total absence of structure, as to which authorities

generally agree, is a point to which I wish to call attention, and to which I shall have again to refer. The semi-fluid state depends apparently on the quantity of water it contains, and protoplasm really exists of various consistencies up to the solid; and, in passing, I may just refer to the very great tenacity of life it exhibits in this last (solid) state. For instance, some of the rotifers can be dried, and after being kept in that state for years will, on being put into water, soon regain their original form and vitality and swim about merrily, doubtless with a happy unconsciousness of the lapse of time since they last so disported themselves. But the most striking illustration of this tenacity of life occurs in the vegetable kingdom—in seeds, for example—a most notable instance of which is mummy wheat, which, after lying by for many centuries, on being sown in the ordinary way, grows naturally without any indication of loss of power from its long rest. Of course these seeds, as well as the dried rotifers, must have retained life through all these years. There was no death, but only suspended animation. It is, however, in the semi-fluid state of which I have spoken that protoplasm can be most readily obtained and best studied. Its chemical composition is said to be very complex, but not to have been exactly determined. Its principal elements, however, are oxygen, hydrogen, carbon, and nitrogen. But besides chemical elements there is something else which is beyond the range of chemical analysis, and that is Life. What Life is we do not know—whether it be simply a property of matter, or something different from and higher than this. All we know certainly is that it is that which distinguishes protoplasm from every other substance, which gives to it its peculiar properties, and which enables it to control and modify the chemical affinities of the other constituents. Life is essential to protoplasm, and it therefore seems to be a mistake to speak, as some do, of dead protoplasm. When life has gone it ceases to be protoplasm; all we have left being the ordinary chemical elements subject to ordinary chemical laws. This, however, is a question which it forms no part of my purpose to discuss. I simply give my opinion for what it is worth, and will pass on to the consideration of the first of the powers of protoplasm to which I wish to call attention, viz., the power of assimilation, *i.e.* of converting pabulum or food into protoplasm precisely similar to itself—non-living into living matter possessing all the powers of the living matter which produced it. This may, at the first glance, seem to some to be merely a chemical operation, similar to others with which we are familiar, in which various substances act and re-act upon each other according to certain fixed laws. But there is in reality no resemblance between the two; for I believe that there is no instance in which by chemical action any substance, simple or compound, converts another substance into one precisely similar

to itself, the result of chemical combination being a substance differing from any of its separate constituents. If an acid—say acetic, is brought into contact with an alkali—say potash, the two unite and a compound, acetate of potash, is formed, which differs materially from both the acid and the alkali; and if this compound is brought into contact with an acid for which the alkali has a stronger affinity than for acetic—say nitric, the alkali will leave the first and unite with the second acid, forming a new compound—nitrate of potash—quite different from either of its constituents; and so we may go on through endless chemical combinations more or less simple, but in none of them is there any conversion of one substance into the other; the resulting compound, as I have already said, invariably differing from its constituents. Chemists have rather recently been able to produce in their laboratories compounds which had hitherto been supposed to be produced only by living bodies, but these compounds are non-living. No chemist has been able, or probably ever will be able, to manufacture a single particle of living matter, and the fact remains that to protoplasm alone belongs this power. We cannot tell how this transmutation is effected in the absence of mouth, stomach, secretory organs, or indeed organs of any kind, there being nothing from which we can obtain any information, or which will give us any hint as to the *modus operandi.*

There is another point in connection with assimilation to which I may briefly allude, and that is the power of sensation possessed by protoplasm. On being touched by a particle of suitable food the soft matter of an amœba flows round and engulphs it with a view to assimilation. Now, this action of the protoplasm, for of such the amœba entirely consists, indicates a power of receiving impressions from without and acting upon them; for it is evident that, were it not so, the touch of an external object would not be responded to, and the protoplasm would remain inert. It is indeed asserted that contact is not always necessary, but that a particle of food in the neighbourhood of an amœba can exercise a stimulating effect upon it, with the result of pseudopodia being thrust out for its appropriation. This, however, is perhaps a little doubtful; and, though my own observations have in some cases seemed to confirm it, I am not satisfied, and think the verdict at present must be non-proven. But however this may be, the fact of protoplasm receiving and responding to impressions from external objects remains; and we thus have it, without nerves or other apparent organisation, exercising a power which in higher animals requires a nervous system more or less elaborate, which again implies something more than is visible even with the microscope.

Let us now pass to another power, viz., that of motion, *i.e.* its

self-motive power, not only from one point to another, but also of its atoms or particles among themselves. This, too, at first sight may appear to be a simple matter, but if we think about it we shall find that it is not so, but that it suggests considerable complication. When I have asked for an explanation of it I have often been answered, "Oh, it moves because it is alive;" but this can scarcely be regarded as a satisfactory reply. We may have a pressure of steam in a boiler, but to utilise it suitable machinery is requisite; and the same argument applies to every other force, including that of vitality.

The simplest form of protoplasm having an independent existence is the moneron, which is a speck of protoplasm without any differentiation whatever. But next in simplicity to this, and more readily obtainable, is the amœba, which is so far differentiated as to possess a nucleus and contractile vesicle. The protoplasm forming the outward boundary is apparently a little firmer than the rest, but beyond this it is as devoid of structure as the moneron. Watch one of these amœba wandering about in search of food—flowing about would perhaps be a better term—and you will see that it has no settled form; it is continually changing its shape from moment to moment, thrusting out pseudopodia in all directions and retracting them with equal ease. The particles of its semi-fluid substance move freely about amongst each other, with a constant change of their relative positions, every particle of the creature being seemingly independent of every other particle, but still to work harmoniously with it. Now, if I wish to extend one of my limbs, or to make any movement of the whole or any part of my body, an elaborate system of nerves and muscles is brought into requisition. The necessary stimulus must be transmitted from the brain through the nerves to the muscles, causing the contraction of these so as to produce the intended movement. But the amœba, without any brain, nerves, or muscles, or indeed any organisation that we can detect, moves, as I have said, in any and every direction, changing not only its position as a whole, but also the relative position of its individual particles, a power which we cannot conceive to exist in the absence of organisation. Then, again, there is that curious movement of protoplasm known as cyclosis, which occurs in the cells of various plants. Some, indeed, think that it occurs in the cells of all plants at some period or other of their existence, though it has hitherto been observed in comparatively few. It must not be confounded with sap circulation, which takes place in the spaces between the cells, cyclosis being within the cells. One of the most beautiful and easily-observed example of it is in the Chara, especially in *Nitella Translucens*, a moderate amplification, say 50 to 100 diameters, sufficing to show it clearly, the cells being comparatively large. The protoplasm may be seen travelling longi-

tudinally from end to end of the cell and back, its course rendered more evident by the starch and other granules carried along by the stream, being up one side and down the other of a spiral line marked by the absence of the chlorophyl grains which line all the rest of the cell wall. Similar cyclosis may be observed in the Desmid *Closterium*, and may be most readily detected at each end of the cell; but a higher power is required than for Nitella—say about 400 diameters, and with a still higher amplification—700 to 800 diameters, and with careful illumination and focussing it may be seen just under the cell wall in any part. In the leaf cells of Anacharis, Valisneria, and some other plants, the cyclosis is nearly longitudinally round the cells, and a power of 300 to 400 diameters shows the granules of chlorophyll being carried along by the stream in those cells in which the granules are few; and in those cells in which the granules are too closely packed to allow of such movement, a higher power will show the protoplasm itself in motion. In the moniliform leaf-hairs of Tradescantia, and in some other plant-hairs, the movement is somewhat different. In them we have a nucleus from which threads of protoplasm proceed in various directions, sometimes meeting and uniting with each other, and after somewhat irregular movements finding their way back to the nucleus. This seems to continue incessantly during the life of the cell, and to see it distinctly a power of not less than 400 diameters should be used. We have also the curious movement of living diatoms, many of which move to and fro in water as if possessed of consciousness; and what is perhaps more puzzling, they also turn on their longitudinal axis; and in none of these cases, either movements of diatoms, or the cyclosis of which I have spoken, can we find anything which will render the cause apparent. Various theories have been propounded, especially with respect to the movements of diatoms; but none have been accepted as satisfactory, and the riddle is still unsolved. Leaving these, and going a little higher up in the scale, we come to the ciliated infusoria, and it is to their cilia I would now call attention. We find cilia even in the highest forms of life, but it is in these low organisms, which are little if anything more than ciliated cells, that they are most prominent and play the most important part, their principal uses being for obtaining food and for locomotion, though there are of course numerous exceptions to the latter use, many ciliated organisms being fixed. These cilia are generally regarded as simple extensions of the protoplasm, though Dr. Beale and others think that externally they consist of "formed matter," but, as far as their motion is concerned, this perhaps is of little importance, but the manner in which cilia are sometimes produced would seem to favour the general opinion. I allude to what we see under the microscope when such an organism as a paramecium is multiplying by fission.

This operation, which probably many of you have witnessed, begins by a slight constriction appearing about the middle of the body, and this goes on gradually increasing until the two ends are almost separated, when it is most interesting and amusing to see the tugging which takes place between the two halves with a view to entire separation; we see also threads of protoplasm drawn out between the adjoining ends, and, when at last the final tug is given, these threads break and leave each of the two new paramecia duly provided with cilia, and so fitted to begin life anew, on a reduced scale. Now in the case of these ciliated infusoria there is a total absence of nerves, muscles, or other visible organisation which accounts for the movement, and yet we have these cilia vibrating rapidly to and fro producing definite currents in the water, the principal one being in the direction of the mouth, and thus bringing the necessary supply of food. It is evident that to produce such a current it is not enough that the cilia should move, but that they should move in a definite manner, and in harmony with one another. And similarly with the Flagellate infusoria, monads and similar organisms which swim gracefully through the water by means of flagella, the motion, to be of use either as a means of locomotion or of procuring food, must be of a definite kind —and we find that where flagellates form colonies, as in the *Volvox globator*, and others, the flagella of the individuals vibrate in harmony with each other as is evidenced by the regularity of the current produced, united action being essential to such regularity. It has been suggested that this vibratory movement of cilia is caused by the alternate contraction and expansion of their opposite sides—one side expanding while the other contracts, and *vice versa;* and this is, perhaps, not an unreasonable explanation, but it is not a complete one; the question still remaining as to the cause of the contractions and expansions. The same explanation might apply to flagella, but the objection would apply still more strongly, their movements not being so simple apparently, as those of cilia. They frequently move in a sort of spiral or corkscrew fashion, and, moreover, are often used for prehensile purposes, much the same as the trunk of an elephant. With one of these flagellated infusoria—a fixed one inhabiting a beautiful tiny transparent vase— which I saw some time since, I was very much struck; its mode of projecting and retracting its flagellum differing from anything I had seen elsewhere, and its singularity will perhaps be considered a sufficient excuse for my referring to it. When retracted, the flagellum was coiled up into a regular circular volute, which, on being projected, uncoiled much in the same way as a straight ribbon of well tempered spring steel similarly coiled would have done, but much more slowly and gracefully. This, on the theory I have alluded to, would indicate a very curiously modified power

of contraction and expansion of this slender thread of protoplasm; for remember, these organisms are simple cells so far as we can see. The simplicity of structure of these infusoria is shown in many ways: for instance, by the beautiful Paramecium, Bursaria, in which the cyclosis of the protoplasm may be seen very much as in the leaf cells of valisneria and some other plants. And thus we have another instance, in the movements of cilia and flagella, of an effect being produced without any visible adequate means.

We will now leave this, and consider the power of construction possessed by protoplasm, which, embracing as it does all things living, both animal and vegetable, is much too large a subject to be taken in its entirety; so with respect to the greater part of it we may content ourselves with the fact that all living forms from the amœba to man, and from the tiniest fungus to the stateliest forest tree, are built up by the direct or indirect action of protoplasm: all arise from that primitive cell of which the amœba is the type. I would rather now call your attention to the structures or homes which protoplasm, in its simplest forms, makes for itself. Among them are those of the *Difflugia proteiformis*, and similar low organisms. They are simply somewhat irregular, round, or egg-shaped cells of a hyaline film, or foundation covered with fragments of earthy and other matters, which have probably been picked up in the neighbourhood. At one end is an opening through which the occupant (which seems to be an amœba with a taste for housekeeping) pushes out pseudopodia in search of food, or for purposes of locomotion. The Arcella, an equally structureless creature, constructs a neater habitation, in fact a really pretty one, generally of a rich brown colour, and dotted, or reticulated like a diatom, much the same shape as a Tam o' Shanter hat, and with an opening similarly placed to that in the hat, through which the occupant thrusts its pseudopodia, and so communicates with the world. I might go on with other illustrations of gradually increasing complexity, not forgetting the elegant tubes and vases constructed by some of the flagellated infusoria, but will pass on, with this simple reference to them, to those beautiful objects, the diatoms of the vegetable and the polycistina of the animal world. In diatoms we find minute specks of endochrome (the active living part of which is protoplasm) enclosed in silicious cells of the most varied shapes—sigmoid, circular, triangular, square, &c., of great delicacy, and many of them ornamented with regular and beautiful patterns in lines and dots—dots or beads principally—high powers frequently resolving into beads the lines shown by lower ones.

Some of the circular diatoms, such as Arachnoidiscus and Heliopelta, are extremely beautiful: the former especially is suggestive of a fine circular window. Diatoms increase in the same

manner as other algæ by the elongation and division of the cells; but we must remember that the diatoms, as we usually see them mounted, are the ends or septæ, and that it is the side or hoop which connects these, that elongates. What interests us most at present is the fact that in building up these cells the protoplasm has not only to obtain from the surrounding water the materials requisite for an increased quantity of endochrome, but also the silex or flint for the cells or boxes as we have called them. But, inasmuch as these diatoms have a cell wall in which the deposition of silex takes place, the curiosity of their production is much exceeded by that of the polycistina in which this is absent, and which, it seems to me, is one of the most striking examples we have of the kind of constructive power of protoplasm we are now considering. You are doubtless all acquainted with their beautiful forms either from observation, or from illustrations you have seen, not forgetting those finished ones shown last year by our good friend (Mr. Williams) in illustration of his interesting paper on Silex. These shells, as we may call them, are very minute, with comparatively large perforations, giving them a reticulated appearance, and many of them have solid silicious prolongations symmetrical, and curved angular or branched. In some cases there is one silicious shell within another, with these prolongations originating in the inner one, and passing through both. They are very widely distributed, having been found on nearly every ocean floor. They are also found in thick deposits of the last geological periods. Now these shells are tenanted by specks of protoplasm which have constructed them, extracting the necessary silex from the surrounding water, and when we remember the infinitesimally small quantity of silex which water can hold in solution we may form some faint idea of the wonderful nature of the process. It is very different from the construction of ordinary shells, which to a considerable extent represent the shape of their occupants; for here we have shapeless specks of protoplasm building up of most refractory material structures of the most varied forms, but always symmetrical and beautiful—another instance of what unorganised protoplasm can do. There are several other powers of protoplasm well worthy of notice, but I will content myself with referring to only one of them, that one however being, perhaps, more wonderful and incomprehensible than all the others, and, therefore, more interesting, and that is the power of transmitting individual properties, characteristics, and peculiarties from one generation to another; a power which is exercised through the whole animal and vegetable kingdoms from the lowest to the highest forms, and upon which rests the universal law that like produces like. The amœba wanders about appropriating and assimilating pabulum until it attains a certain size, or rather an uncertain one, as they attain

very different sizes before dividing; but when this is reached it divides itself into two smaller ones, each similar to and possessing the same qualities as its predecessor. So with the lower ciliated and flagellated infusoria to which I have already alluded, and which seem to belong to either or neither kingdom, the same rule obtains; the cell dividing into two smaller ones, which grow and in their turn divide; the organism never changing to anything else, and the case is the same when they multiply by another mode than that of fission. And so as we ascend the scale of life we find it is the same: there is no change. The primary cell of an oak never produces anything but an oak, or that of a dog anything but a dog, and so we have the likeness perpetuated. Not only, however, are the broad general features of the plant or animal reproduced, but also, in many cases at least, the peculiar characteristics of the individual, and these, if beneficial, *i.e.*, favourable to the welfare of the individual, after transmission through many generations, having meantime been intensified by natural selection, become permanent, and so tend to the establishment of new species. In the animal kingdom not only are physical qualities transmitted, but instinctive also; might we not say mental as regards some of the higher animals? At all events, instincts are transmitted in a remarkable degree through the whole of animal life except, perhaps, the very lowest, and we might even find the same things there had we the means and opportunity of observing with sufficient accuracy. On the principle, however, that the greater includes the lesser, let us take this question of transmission in connection with the highest animal—man—and see if we can in any way understand or explain it; if we can solve the difficulty in his case, we can readily do so in all others, and there are also other advantages in so taking it. The most common and most readily observed of physical characteristics transmitted is the resemblance of the offspring to one or both parents in feature and form, which, in some cases, is very marked, and accompanying this, or without it, little tricks of manner, peculiarity of gait, and so forth, are frequently inherited. In some cases, of course, these last may be the result of imitation, but not always, as they are known to occur where the opportunity for imitation does not exist. Then various personal deformities are frequently transmitted: for instance strabismus, and various diseases, or the tendency to them, as gout, epilepsy, &c. We well know, also, that the mental peculiarities of one or both parents are often inherited by the offspring in a remarkable degree, such as aptness or talent for music, painting, literature, science, and other things, for though this is not universal its occurrence is sufficiently frequent to establish it as a fact. We may even go further, and say, without much fear of contradiction, that

moral qualities are frequently transmitted in like manner, and doubtless many of you can call to mind instances in which peculiarities of temper, or still worse, the tendency to drunkenness or other vices have almost certainly been inherited. It is, indeed, held by many, with respect to what are called the criminal classes, that the tendency to crime in their younger members is not altogether the result of their surroundings and education (or want of it), but is due, to a great extent, to the inheritance of vicious tendencies from their parents. Be this as it may, there is no doubt as to the reality of the transmission of physical, mental, and moral qualities from one generation to another, and, this being so, let us consider what it involves, or what explanation can be given of it. Well, the first attempt at explanation is, that all the characteristics and qualities of the offspring, be they similar to those of the parents or not, are directly impressed upon it by its Creator, either at once, say at its birth—or more gradually—not only then, but before and after that event. Some years since this view, no doubt, obtained very largely, and there are still a number of persons who accept it, especially among those who are in constant fear of science clashing with, and, mayhap, overturning beliefs which they hold (and very conscientiously too) to be essential to religion. Some attribute everything to education, and go so far as to suppose that all human beings come into the world on exactly the same footing, and without any inherited tendencies, the mind of an infant (I do not know if they extend this view to animals generally) being a *tabula rasa* on which anything may be written according to the will and fancy of the writer. People holding this view are now few and far between, and it is too absurd to be worth consideration. Nor will the teleological explanation referred to be satisfactory to the scientific man. He knows that effect follows cause, and that for every effect, no matter how small or great, there must be an adequate cause, and not only so, but that there must also be adequate means by which the cause can produce the effect. To make the bell ring there must be, not only the power applied to the bell-pull, but there must be the wire to carry the impulse to the bell. If, then, we reject the teleological explanation, what is our alternative? It is simply this, that, in some way or other, all the parental characteristics and peculiarities, physical, mental, and moral, which are destined to be reproduced in the child must necessarily be contained in the fertilised ovum from which that child is to be evolved, and thus be in a position to be transmitted to it. It may be said that this cannot be absolutely necessary as regards the characteristics of the female parent, as there is opportunity for the gradual infusion of these into the offspring during the period of gestation, which may be true, but is scarcely supported by analogy, as in the case of birds and other

ovipari, it is evident that the ovum must represent both parents; and so, I would infer, it is with the human ovum. Be this however as it may (as respects maternal) it is certainly true that the paternal characteristics must be contained in the fertilizing cells, many thousands of which would go to make up a single grain weight, and be by them conveyed to the ovum. Thus we have a cell about the one hundred and twentieth of an inch in diameter, fertilised by cells almost infinitely smaller, which eventually develops into the perfect child; and the speck of protoplasm in this cell must contain and transmit qualities, peculiarities, and tendencies, physical, mental, and moral, which may largely affect the character, and consequently, perhaps, the destiny also, of the child—the most wonderful *multum in parvo*, I venture to say, which can be found within the whole range of human knowledge. The transmission of physical qualities is beyond our comprehension, how much more that of mental and moral. I think I am not wrong in speaking of this power as the most wonderful one possessed by protoplasm, and I must confess that when the full meaning of it first flashed across my mind (for I had only seen it referred to in a casual way, not calculated to attract much attention) I felt completely overwhelmed—just as one is by the idea of time and space without beginning or end, an idea too vast for the human mind to grasp—and much thinking over it since has scarcely diminished that feeling. There are many interesting questions which arise out of this subject which we cannot discuss or further refer to now. This view of transmission by protoplasm may by some be considered thoroughly rationalistic, and it certainly has that appearance, but perhaps is not really more so than views of many other effects of natural laws which are adopted without objection on that ground. At all events I feel bound to accept this view, and do not consider myself a materialist. Having now passed in review some of the most striking powers of protoplasm, let us briefly sum up and see what inference we may draw from them. We have seen that protoplasm has a power of assimilation—of converting other substances into its own likeness non-living into living matter; that it is sensitive; that it can build up the most beautiful structures of the most obstinate and unpromising materials, and that it can transmit hereditary qualities, physical, mental, and moral, from one generation to another; all these operations, and others to which we have not referred, being peculiar to itself and performed without any visible organs or organisation. Professor Allman has said, referring to protoplasm:—"Examine it closer, bring to bear upon it the highest powers of your microscopes. You will probably find disseminated through it countless multitudes of exceedingly minute granules; but you may also find it absolutely homogeneous, and, whether containing granules or not, you will find nothing to which

the term organisation can be applied. You have before you a glairy, tenacious fluid, which, if not absolutely homogeneous, is yet totally devoid of structure." This statement, as to the total absence of structure, is also made by other physicists, and generally accepted. But though our best appliances fail to detect any organisation, can it really be that such is wanting? Perchance it may be totally unlike anything with which we are acquainted, and which the microscope is as little adapted to reveal as it is to pry into the secrets of electricity or magnetism. Wherever we find a result accomplished we naturally, and properly, look for the mechanism requisite to produce that result, *i.e.*, simple or complicated, according to the result accomplished; and though the machinery be hidden we still infer its existence. For the making of a piece of plain cotton cloth a comparatively simple loom is required, but for the production of an elaborately ornamental fabric, a much more complicated one is requisite. Again, if a machine has to perform only one operation—for instance, a lathe only to turn cylindrical pieces of wood or metal—a simple one will suffice; but if, in addition to this, it is required to turn oval, spiral, and other forms, and also to act as a planing, slotting, and shaping machine, it is evident that the machinery must be proportionately complicated. Now, protoplasm performs numerous operations, exceeding in complexity anything affected by machinery made by the hand of man; and, it appears to me that the only inference which can be fairly drawn from this, and it seems a reasonable one, is that so far from being the structureless and unorganised substance which, even under the highest powers of our microscopes, it appears to be, it must be possessed of an organisation, the complexity of which surpasses the utmost efforts of our understanding and imagination.

PRACTICAL PROCESSES IN VEGETABLE HISTOLOGY.

By L. Oliver, in Rev. Sci. Nat., 1882.

(Continued from page 50.)

V. Dissolution and Destruction.

We dissolve certain substances either with the object of discovering what they are, or more frequently the better to see the elements which they hide. Thus it is not uncommon to destroy the protoplasm in order to make the nucleus more visible.

Protoplasm.—In order to display the nucleus the tissue is treated

with acetic acid, which renders the protoplasm transparent, and then dissolves it. A concentrated solution of potash destoys it, but that attacks the nucleus as well. It is only employed to obtain a membranous skeleton of the tissue.

Aleurone.—Sulphuric acid entirely destroys the grains of aleurone.

Oily Matters.—The oily matters have a special refrangibility under the Microscope, which distinguishes them from other substances inclosed in the tissues. Their most general solvents are ether and the essential oils; alcohol, chloroform, and benzene are also often used for this purpose.

The oily matters which exisit in the solid state in plants, and which are known by the name of *vegetable butters* (cocoa-nut butter, cocoa butter, nutmeg butter, Japanese wax, palm-oil, laurel-oil, &c.), may be dissolved, like oily liquids, in ether and essential oils.

The use of alcohol is often recommended to remove the oil from sections of the albumen, the embryo, or the cotyledons of oleaginous seeds; we ought to call attention to the fact that ether acts more rapidly, and that moreover several oils are only partly soluble in alcohol, such as linseed-oil, hempseed-oil, poppy-oil, croton-oil, and nut-oil.

Essential Oils.—These oils are very unequally soluble in alcohol or ether; they are all soluble in the fixed oils. They exist in the tissues in the condition of balsams or oleo-resins. The non-volatile oils, in which the resinous substances are insoluble, allow of their extraction.

But as the use of the fixed oils is inconvenient because of the difficulty of getting rid of them from the preparations which have been impregnated by them, we point out, according to Planchon,* the solubility and density of several essential oils, which it is useful to know in order to free the sections from them.

A. Essential oils denser than water:—Bitter almonds, cloves, mustard, cinnamon.

B. Essential oils less dense than water:—

Camphor.

Essence of roses, soluble in sulphuric acid.

Essential oil of aniseed: when sulphuric acid is added to it in sufficient quantity the solution separates into two layers, of which only one is fluid.

Essential oils of conifers, only soluble in several times their volume of alcohol.

Essential oil of lavender, soluble in one volume of alcohol.

Essential oil of rosemary, mint, and thyme, very soluble in alcohol.

* Planchon, 'Traité pratique de la détermination des drogues simples d'origine végétale,' ii.

Resins.—When examining the oleo-resinous ducts of plants, especially in the Coniferæ, Cycadeæ, Aroideæ, Umbelliferæ, Araliaceæ, Compositæ, and Clusiaceæ, in which they are very much developed, we must eliminate the resins which accumulate in the passages where they were originally united with the essential oils, as has been done by Sachs,* Trécul,† N. J. G. Müller,‡ and Ph. van Tieghem.§ It is the same with the *resins* properly so called (betulin, colophane, jalap, lac, &c.), the *balsams* (tolu, benzoin, &c.), the *gum-resins* (gamboge, &c.). These substances, abundant in the sections of old tissues, generally prevent the study of the oleaginous cells. They can be completely dissolved in the fixed oils by heat. But it is generally preferable to treat them with essential oils, ether, or alcohol, which at ordinary temperatures dissolve the greater portion of them. The little which remains in the passages does not injure the examination of the preparation, and moreover this imperfect solution of the resin, joined to its other characters, helps in its recognition.

Waxy matters.—The waxy matters of the cuticles are but slightly soluble in cold alcohol, but they dissolve very quickly in boiling alcohol or slightly warmed ether. It is the sections themselves which are subjected to the action of these liquids in order to obtain perfectly pure cuticles, or to recognize the waxy nature of the substances developed at the surface of these membranes.

Latex.—In making sections of organs provided with latex, care must be taken to keep the razor and the preparations continually wet with ether. Without this precaution the latex blackens the razor, and consequently the tissues which are being cut, so that it becomes impossible to examine them.

Caoutchouc is composed of the corpuscles of the latex of certain plants. These corpuscles can be recognized under the Microscope by their swelling in the volatile oils, and dissolving in benzene, chloroform, and bisulphide of carbon.

Cellulose.—Cellulose, as it is most frequently present in the cells, that is in the condition of polymerization not exceeding $(C_6H_{10}O_5)_4$, is soluble in Schweitzer's ammonio-cupric solution. More condensed (for instance, elder pith, the walls of thickened fibres, old vessels, ligneous cells) it is insoluble in the same reagent.

Schweitzer's solution alters with time, therefore it ought to be used freshly prepared. It is obtained by pouring ammonia on copper turnings, in a funnel; the liquid is again poured over the copper until it is coloured deep blue.

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* Bot. Ztg., 1859, pp. 177-85.

† Journ. de l'Institut, 6th Aug., 1862. Ann. Sci. Nat., v. and vii.

‡ 'Untersuchungen über die Vertheilung der Holze,' 1867.

§ "Mém. sur les canaux sécréteurs des plantes," Ann. Sci. Nat., xvi. (1872).

As the solution of cellulose can only be effected by a large quantity of nitrite of ammonia care must be taken to keep a constant current of the liquid passing between the two glasses between which the preparation is compressed. For this purpose pieces of filtering paper are used, which absorb the liquid at the edge of the cover-glass, whilst some drops of the solvent are placed at the opposite edge. The operation is hastened by disusing the cover-glass where large sections are being treated.

When the preparations are numerous and resisting they can be shaken together in a little flask filled with Schweitzer's liquid, and subjected to several washings. This is the most rapid process. But if the preparations are at all delicate the first method alone is practicable; the operator should follow under the Microscope the different stages of the solution. The observation is easy with a low power; but directly it requires more than 200 diameters it becomes troublesome. In this case it is better to increase the power of the eye-piece alone; high power objectives are inappropriate, the distance of their front lens from the preparation is so small that they risk being wetted by the reagent.

The butyric fermentation offers a slower but more accurate means of isolating in a preparation all the non-cellulose membrane by determining the cellulose. The organs or the sections from which we wish to eliminate the purely cellulose portions are placed in a glass of water, to which are added pieces of radish-roots, haricot beans, or broad beans, a *very small* quantity of sugar and powdered carbonate of lime. The mixture is shaken up, and left exposed to the air. The fermentation is increased by keeping the vessel in a temperature of about 30° C.

When, carbonate of lime being in excess, there is no further disengagement of gas, the *Bacillus amylobacter* has formed its spore, and the fermentation has ceased; all the cellulose has then been, by a series of successive hydrations, converted into glucose, and the glucose decomposed into carbonic acid and butyric acid. The *rôle* of the carbonate of lime is to allow the formation of butyrate of lime as butyric acid is produced; this acid, free and accumulating in the liquid, would arrest the development of the *Bacillus* long before the destruction of all the cellulose.

Like Schweitzer's solution, the butyric ferment does not attack cellulose whose condensation exceeds $(C_6H_{10}O_5)_4$. The action of the microbe is indeed so special that it is only exercised on a certain kind of this compound, although no chemical reagent shows two varieties of it. Thus cells of *Chara* and *Elodea*, although dissolving in nitrite of ammonia, are not altered by *Bacillus amylobacter*.

Generally this microscopical agent does not affect starch, which is a lower polymer than cellulose. Nevertheless, Van Tieghem

has found that in certain plants, contrary to what usually takes place, this microbe subjects the grains of starch to butyric fermentation, without destroying, or before destroying, the walls of the cells into which it has penetrated. This is the case with the root of *Adoxa moschatellina*.*

It is easy, with a *high magnifying power*, to study, under the Microscope, the course of the butyric fermentation. It is only necessary to guard against the preparation drying up and coming in contact with the air, which is fatal to *Bacillus amylobacter.*

Crystals of Carbonate of Lime.—In the condition of cystoliths, or of very small granular crystals, carbonate of lime is not rare in the protoplasm or septa of the cells (for example, plasmodia of the Physareæ, epidermal cells of several Urticaceæ, cell-walls of *Corallina* and *Acetabularia*). Acids, and particularly hydrochloric acid, dissolve it by disengaging, under the form of bubbles, the carbonic acid which it contains. This disengagement, easily observed under the Microscope, is very characteristic.

Crystals of Oxalate of Lime.—These crystals, which are much more frequent than the former, are distinguished from them chemically by being insoluble in acetic acid, and soluble, without disengagement of gas, in hydrochloric acid.

It is useful to apply these reactions in the case of crystals of the quadratic system with six equivalents of water. But for the raphides of the monoclinic system, with two equivalents of water, they are almost always superfluous, their form being sufficient to reveal their nature.—*J. R. M. S.*

NOTES ON SOME FREE-SWIMMING ROTIFERS.

By J. E. Lord.

SOME time ago an announcement was made, I believe at a meeting of the Manchester Microscopical Society, to the effect that some one was bringing out a work on the "Rotifera." From that time to the present I have heard nothing more about the subject, although many others along with myself have been looking very anxiously for some further information, as a work on this subject is an absolute necessity. I should be glad to hear that Mr. Saville Kent had taken the matter in hand, as we should then have a guarantee that the work would be performed in a manner worthy of the subject. In the preface to his magnificent work on

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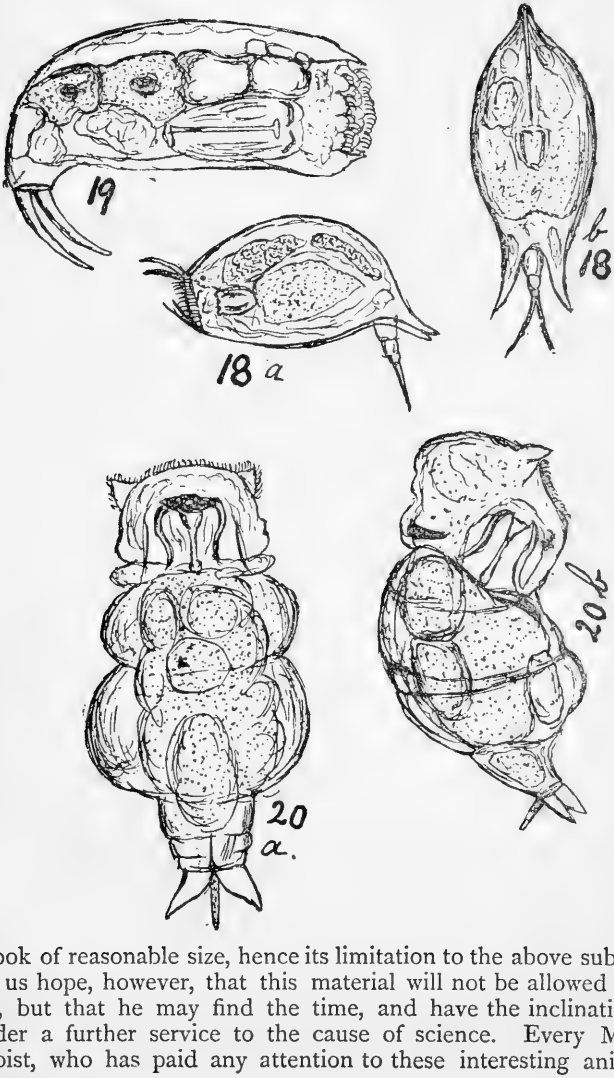
* Van Tieghem, "Anatomie de la Moschatelline," Bull. Soc. Bot., ii. (1880) p. 282.

the ciliate, flagellate and tentaculiferous Infusoria, he tells us that, originally intending the work to run on the lines of "Pritchard" he had collected a vast amount of material on the various subjects there treated—too much in fact to be brought within the limits of



a book of reasonable size, hence its limitation to the above subjects. Let us hope, however, that this material will not be allowed to lie idle, but that he may find the time, and have the inclination to render a further service to the cause of science. Every Microscopist, who has paid any attention to these interesting animals,

must often have found it impossible satisfactorily to identify some of his finds. One has little difficulty with the large fixed forms, but the tyro's troubles begin when he comes to study the more minute free-swimming Rotifers. The "Micrographic Dictionary" is of little service, as it gives descriptions of species in comparatively few instances. "Pritchard," while treating more fully of the various species, is yet very vague in regard to the less frequently recurring forms. It has often been a source of regret to me that the *Microscopical News* has not been made the medium of more information on these charming animals. I trust, however, that these "Notes" may be the means of eliciting information upon a subject interesting to a large section of your readers. The following notes are taken at random from my "Sketch and Note Book," and I shall be glad if they are followed by others from experienced microscopists.

Fig. 18, *a*, side view, *b*, dorsal view. This Rotifer agrees most, I think, with the genus Colurus. Its lorica is cylindrical, compressed, open on the ventral side, and with faint indications of a ridge on the anterior dorsal. This part is also pointed; posterior portion of lorica with two horns. Rotatory organ with two upper hooks, and two lower setæ. Tail-foot, forked; toes, about as long as foot. From a well which contains many diatoms, several other forms of loricated, free-swimming Rotifera and Tardigrada, and which has Mosses and Batrachosperma growing upon its sides. If this is new, which I hardly expect, I propose to give it a name expressive of its general resemblance to a ship.—*Colurus navalis.*

Fig. 19. This is a side-view of a Rotifer I am not able to identify with any known species. It has most, and very strong points of resemblance with Salpina. For example its eye is single and cervical; foot furcate; lorica prismatic, with elevated ridge; and anteriorly it has either 3 or 4 horny processes. It differs only, at least so far as I have been able to make out, in the posterior part not being toothed, and in the toes being decurved; the gizzard too is perhaps rather Hydatinean in its character. It swims about in much the same way, and has a general resemblance to *Rattulus lunaris*, fam. Hydatinæa; but its close affinity to Salpina is undoubted.

Fig. 20, *a*, ventral view, *b*, side view. This Rotifer, which evidently belongs to the genus Hydatinæa, is a very peculiar but very interesting one. Characters as follows:—Animal stout, truncated anteriorly, enlarging from front towards base of foot, where it is suddenly attenuated; foot, cylindrical, plicate, about one third the length of the animal; toes short and stout; one red eye seated on a dark mass; near the mouth is a beak-like projection (shown in side view); jaws protrussile; fine cilia on ventral surface of head; integument thrown into deep plications; strongly ciliated internally; sarcode-like process situated dorsally, just above, and nearly

at right angles to the toes. I had watched it occasionally for several days before I saw it protrude the two ear-like ciliated lappets, which probably represent the wheels. This animal offers many difficulties to a satisfactory classification. It appears to agree most with the *Pleurotrocha gibba* of "Pritchard," but my specimens had an eye—its cilia too are differently placed, and its gizzard differs from the figure he gives. It is a sluggish Rotifer, and frequently remains for some time rolled up like a ball. Several times I have found them greedily devouring those gelatinous masses of vibrio. What is the sarcode-like process? Is the animal a male? If this proves to be the fact our views as to the male Rotifers having no digestive organs will have to be considerably modified, as this specimen has a rather powerful-looking gizzard, which I was not able to study sufficiently to be able to give a correct description. I have now given sketches and description of three species of the Rotifera, about which I shall be glad to obtain information, and in a subsequent issue of your journal I may recur to the subject, giving sketches of others, and in one or two instances of peculiar forms.

INTERNAL PARASITES OF THE DOG.

By Harold Leeney, M.R.C.V.S.

IN order to clear off some of the "dust" which a correspondent speaks of as being thrown in the eyes of dog-owners, I propose now to speak of the *known* origin of many kinds of worms, as they are commonly called, and of the *probable* cause of others, and by this means sweep away some of the accumulated rubbish with the besom of scientific fact, borrowed from Dr. Cobbold in great part, whose researches have justly gained him the first position in the helminthological world.

In treating of skin diseases we have already mentioned the principal external parasites, many of which bear no resemblance to worms, and in order to approach so large a subject as parasitism, it is well at the outset to discard the idea of a worm being either long and round, or long and flat, as they vary from tiny microscopic objects to creatures of many feet in length, and are of all shapes and sizes. Neither do they all inhabit the stomach or intestines; different classes find their most suitable homes in different parts of the body, and I think there is no one part of the animal able to claim exemption from the attacks of one or other of the almost innumerable host. There is a parasite whose delight it is to make

a residence in the dog's nose *(Pentastoma tenioides)*, and another who takes up his abode in the walls of the heart *(Filaria immitis)*, while a third passes a part of his existence in the brain of the sheep, and at another stage in the dog's bowels; others have existence in the connecting parts of the bowels of hares and rabbits, and develop a further stage in the dog that eats them, and so we shall find that man and animals keep up a continual round of reproduction, although some mature parasites require three or four *hosts* or intermediary bearers to complete their development. The *Trichina*, of which we have heard so much of late years, cannot find a home to its taste until it has bored its way outwards from the stomach and intestines, and lodged in the muscular parts of the body. The fluke again, whose disastrous effect upon sheep is well known as the rot, seeks for its home the bile ducts of the liver; while the horse worm, affecting calves, is most at home in the bronchial tubes. The *Spiroptera sanguinoleatea* is a minute worm swimming about in the blood, and another kind of worm sets up a nest at the junctions of blood vessels. Another worm, ¾in. long, is happy nowhere but in the aqueous humour of the eye, while the largest strongle *(Strongulus gigas)* is found coiled up in the kidney. It will be gathered from the foregoing remarks—and they are incontrovertible—that internal parasites have a very wide distribution, and the term *worm* fails to describe any but two or three of the best known varieties. In treating of worms in the dog we shall have to refer to their relations to man, since some of them are common to both, while others pass one stage of their existence in man and another in the dog. When the enormous power of reproduction is considered—one tapeworm segment alone bearing 30,000 eggs—it is not marvellous that so many dogs (and men too), suffer from worms, but rather a matter of surprise that any animal escapes the presence of such infelicitous guests.

Of the round worms infesting the dog the *Ascaris marginata* s the most common. The males are from 2in. to 3in. in length, while the females attain to 4in. or 5in. From experiments made at Vienna it was ascertained that so large a proportion as 104 out of 144 dogs contained this species of lumbricoid; while experiments in England lead to the conclusion that at least 50 per cent. of dogs suffer from the same cause. These worms generally live in the small intestines, but are known to wander into the stomach, and give rise to inflammation of that viscus, and the other symptoms which we have already described as gastritis. They are occasionally passed out of the rectum either by the presence of something obnoxious to them in the anterior part of the canal, or from a voluntary wandering which sometimes leads them so far out of their natural element as to be found in the throat and nostrils. Their presence in the intestine generally produces nausea, which is

manifested by dribbling of saliva, and a dull oppressed appearance, colicky pains, unthrifty coat, hide bound, uncertain appetite, sometimes voracious, and at others refusing food, vomiting, purging, and general loss of condition. Puppies are much more subject to these worms than adult dogs, though they are in no age exempt from them. The breath of the dog is often very disgusting, and attributed to indigestion from other causes, bad teeth, &c.; a husky cough, irregularity of the bowels, and temporary paralysis, may also accompany their presence.

The manner in which this worm is reproduced does not seem to be clearly made out at present, and I was inclined at one time to think the larval form could be received through the medium of the mother's milk, since I have seen puppies four weeks old with bundles of mature worms of this species. I sent a specimen of this kind to the Royal Veterinary College for Dr. Cobbold's examination, but he is of opinion that the larval form must have entered the pups from without, and not through the medium of the mother's milk treatment. Having ascertained that a dog is the unfortunate host of these worms, there is not much difficulty in getting rid of them—the remedy is santonine. All the varieties of round worms, that is to say those who infest the alimentary tract, are readily expelled by this remedy, whether they be the guests of man or dog. A dose of from one to three grains should be given in from one to four teaspoonfuls of olive or castor oil, the dog having previously fasted for at least twelve hours, twenty hours being better. The absence of *water* has not generally been insisted on, but my experience, both of dogs and horses, goes to prove that anthelmintic medicines act better when the stomach and intestines contain the least possible quantity of both solids and fluids. A second dose two or three hours afterwards is advisable to kill and expel any that may have escaped the first dose. Whether or no uncooked cow's milk has anything to do with the production of this species of worm I cannot say, but I have observed that puppies so fed have been the frequent subjects of them, as also of rickets and dropsy; and while speaking of hand-reared puppies I would advise the admixture of lime water with the milk, in the proportion of one part of lime water to two parts of milk, previously boiled. It is not generally known how simple and inexpensive is the preparation of lime water, or its valuable properties would be more generally made use of. It is only necessary to obtain a lump of fresh-burned lime, weighing 3oz. or 4oz., and drop it into a gallon bottle of water, agitating it occasionally, to produce a gallon of proper lime water, filtering it afterwards, or pouring off the supernatant fluid, after allowing the lime to subside.

A worm much resembling this, but with some hair-like pro-

cesses, is the one most commonly found in cats; it is called *Ascaris mystax*, or the moustached worm, and may be expelled by the same remedy as recommended for dogs. Since poor pussy has but few friends, I hope my readers will adopt the harmless and effectual remedy of 1gr. of santonine when a cat, apparently well fed and cared for, persistently loses flesh, and shows those other symptoms which in the more favoured dog would give so much alarm to the owners.

NOTES IN REFERENCE TO STICKLEBACKS' NESTS.

By Thomas Bolton, F.R.M.S.

WHEN Mr. Wilkins read his paper before the Birmingham Natural History and Microscopical Society I made an appointment with him to go in search of some Sticklebacks' nests, and in due course brought home several, one of which was placed in an aquarium in the window of our room at Mason's College. The male Stickleback was soon reconciled to his new quarters, and diligently fanned his restored nest, from which in due course we estimated that he hatched out a brood of at least 250, making quite a populous tribe for a small aquarium about 12 inches diameter.

Watching the nest and the doings of the Stickleback furnished much amusement to the members of the Society and other visitors to the room, and the eggs and young fry were often examined under the microscope to elucidate their development. I am afraid, however, that he was not sufficiently fed, or possibly he may have resented the frequent interference with his large family, for before long he commenced a raid upon them, and sad to relate, swallowed them all up with the exception of one little one, which managed to secrete himself amongst the weed and algæ from his unnatural father. Not long after—we don't know why—he succumbed himself, leaving the aquarium as a home for his single descendant, who afterwards thrived well under the care of the curator, who occasionally fed him, or provoked his pugnacious nature for the amusement of visitors.

In the middle of April last, at a casual visit to the room, Mr. Wilkins at once noted that this little Stickleback had put on the soldierly colours of various bright hues, characteristic of the male Stickleback at the time of maturity, and a closer inspection of the aquarium revealed amongst the weeds a fully formed nest, elaborately

built of algæ, of which he seemed very proud, and pugnaciously repelled any interference with which it was threatened.

Mr. Wilkins came to me and arranged to go in search of some female fish as companions for him, and the next day we readily selected, from his father's native pool, several stout ladies for his companionship. On our return we placed one of these in the aquarium with him, and it was very instructive and amusing to see his excitement. He directly nosed up to her, and giving a kind of knowing twist, sailed back to the mouth of his nest, and this he continued to do repeatedly, evidently intending to show her the way to the nest. Unfortunately, however, whilst we watched his antics, she did not in any way respond to his blandishments, either from not being quite ripe for the deposition of her eggs, or possibly from a relapse of energy resulting from the rough treatment she had endured in her transfer from her natural habitat. However, when I examined the aquarium next morning, I was pleased to see that she had deposited her eggs in the nest of her mate, and that he, with wonderful instinct (how did he learn this?) was diligently fanning the nest so as to keep up a current of water to aërate the eggs. The next and the following evening receptions had been arranged by our Mayor to be held in the Council House, at which the members of our Society had promised to assist with a display of microscopes, so having covered our gentleman over with a cloth and carefully slung the aquarium to a pole, I started, with the assistance of a liveried caretaker, to transfer him and his nest to the Mayor's parlour, where, on uncovering the aquarium, I saw that he was continuing his diligent attention to the nest, and I am glad to say he behaved well, and created much interest with the guests at the successive receptions.

I should not omit to record that the care we took in his removal caused some little excitement among the cabbies and others on the watch, as that morning the discovery had been made public of the dynamite manufacture which had been going on in Birmingham, and evidently, by the wide berth we were allowed, we were suspected of carrying some of Whitehead's handywork.

The aquarium, on its return to the room, was placed in a window, which received a fair share of what little sunlight this cold month afforded us; and on the fourteenth day after the deposition of the eggs the young fry made their appearance, and a small family rewarded his exertions. Perhaps it might have been better if we had allowed him another female or two to stock his nest, as in nature the male Stickleback indulges in a plurality of wives if his first wife does not fill his nest with eggs to his satisfaction.

THE PREPARATION OF POLYCISTINA.

By E. Van den Brock.

THE liquid containing the specimens is first of all strained through a small bag of fine cambric, which is then gently shaken in a vessel of pure water—the latter being removed and renewed several times. Three or four such washings leave a residue, which is either very rich in Polycistina, or contains nothing else. This residue is then boiled in a glass flask with a dilute solution of caustic potash. Generally after five or six minutes of ebullition, the liquid appears full of minute air-bubbles which hold the frustules in suspension. The addition of a little nitric acid, however, immediately clears the liquid so that the Polycistina fall to the bottom of the flask, and the work of washing is rendered easy. After two or three washings in pure water, the residue is boiled anew in strong pure nitric acid for some minutes, the neck of the flask being inclined so as to avoid loss from projection. This operation leaves the Polycistina free from all impurity, and two or three washings with water remove all traces of acid. The whitish residue is then transferred to a watch-glass containing a little distilled water.

If this residue should not be pure but contain grains of quartz, etc., they may easily be removed by levigation.

With the aid of a pipette, or, better still, a little syringe, which can easily be made from a piece of glass tube with the end drawn out fine, and a rod with cotton wound round the end for a piston, a small quantity of the water in the watch-glass containing the residue is transferred to a slide. With a fine pencil, or a needle set in a handle, the Polycistina can be carefully spread out or set in some kind of regular order—provided the quantity of water put upon the slide has not been too considerable. When the frustules are arranged as wished, the slide is held above the flame of a spirit lamp, and the water evaporating leaves the specimens dry in the position in which they were placed on the glass. While the glass is still warm one or two drops of essence of turpentine are allowed to fall upon it from a very fine pipette. Before this dries a little very fluid Canada Balsam is dropped upon the slide. This spreads itself over the objects, and guided by the turpentine, makes its way so completely into the pores and crevices that not the least bubble of air is left in a single Polycistina. It only remains to add a cover and dry the slide, which made in this manner will always be perfectly successful. This mode of preparation was suggested by M. Rutot, who employed it successfully for diatoms and other silicious organisms.

N.B. It is best to give the drop of water containing the frustules

on the slide as great a surface as possible, and to make its edges extend beyond the covering glass, otherwise slight traces of crystallizable matter may serve to indicate these edges, and perhaps, to some extent, impair the beauty of the slide.—*(Bull. Soc. Belge de Micr.)*

NOTE ON THE PREPARATION OF DIATOMS.

By E. Guinard.

A GREAT deal might be done to extend the study of diatoms if each collector were to communicate to his fellow workers the different *tours de main* that experience may have suggested to him. Applying this to myself I proceed to give a most easy method for preserving certain marine diatoms which are generally found in small numbers, and are of a nature not sufficienttly silicious to withstand long boiling in acids and repeated washings without considerable loss to the stock.

When good fortune has thrown in my way an alga containing diatoms in small quantity I employ the following method to guard against all chance of loss.

The marine alga is introduced into a tube from 10 to 12 centimetres deep, filled three parts full with soft water, which is raised to boiling point just for an instant. As soon as the liquid has cooled the whole is well shaken—the frustules filled with their endochrome, turned green by the heat to which they have been exposed, fall to the bottom of the tube. This first washing is replaced by another with distilled water, when, after standing for some hours, a decantation carefully made leaves a small deposit of diatoms of a greenish colour. From this deposit a minute quantity is removed either by means of a pencil, or better still, with the aid of a thin glass tube.

Upon the cover, previously cleaned, is placed a drop of distilled water, to which is added that portion of the deposit removed with the pencil. I evaporate at a gentle temperature, and then calcine at a whiteish red heat upon a sheet or platinum. The mounting is finished in the usual way, either dry or with balsam. I employ this method daily for the preparation of Pleurosigma—many of the frustules of which could not stand prolonged boiling in acid.

When in a botanising excursion I have gathered some specimens of Pleurosigma, I turn them out immediately upon my return into a glass of greater height than width, so as to get a thicker diatomiferous deposit. After standing for from 24 to 48 hours I pass a hair pencil several times lightly over the surface of the

stratum—washing the pencil after each operation in distilled water. The frustules are then boiled as above, and a single washing removes all trace of salt. After calcining at a red heat the specimens are mounted dry in the usual way.—*Bull. Soc.. Belge Microscopie.*

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Tubbs, Brook, and Chrystal, 11, Market-street, Manchester, to whom also all applications for advertisements should be made.

All matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, The Willows, Fallowfield, Manchester.

Errata.—In our last number for *Alteration* of Generations read *Alternation.*

Cole's Studies.—Mr. Cole writes us as follows:—"I am very scrupulous about even the appearance of taking credit for any work I have not done myself, and therefore write (in addition to my *thanks*) to ask you to be so kind as to say in your next number that the "Poppy" essay and the Botanical Section of the Studies, Vol. II., is written by David Houston, F.L.S., and the Animal Histological Section by Mr. Fearnley; the drawings having been made by Mr. E. T. Draper."

Aylward's Pond-Collecting Apparatus.—We have received from Mr. Aylward a very cheap and effective pond-collecting apparatus which certainly should be in the hands of every collector. It consists of a pond-strainer, a net-ring, a small case of corked tubes, a weed-knife, and a bottle-holder. The method used for attaching the various portions of the apparatus to any ordinary walking stick or umbrella is very ingenious and simple, and we feel sure will be largely adopted. The apparatus needs only to be seen to be appreciated by all practical naturalists.

Mr. Bolton's Portfolio.—No. 10 of this important collection of drawings is before us, and we are pleased to find that two of the animals figured therein are new to science—*Chilomonas spiralis* and *Asplanchna Ebbesbornii*. We are not aware that there is any records of *Raphidiophrys elegans* having been found in England before, or that *Hemidinium nasutum* has been previously figured.

A good account of the Raphidiophrys is given as an extract from Dr. Leidy's "Fresh-water Rhizopods," Mr. Bolton telling his readers that he has lately found a fair quantity of these organisms near Birmingham. A good account of *Hemidium nasutum* may be found in Saville Kent's "Manual of the Infusoria." This organism, together with *Chilomonas spiralis*, has been found by Mr. Bolton amongst decaying leaves in Sutton Park. He says:— "In going yesterday to collect the Hemidinium for despatch to-day, I found in the ditch nothing apparent but dry dead leaves, but under these in the mud was a little water, which, on my return home, proved to contain an abundance of Hemidinium, but there is a larger proportion of other larger Infusoria and some common Rotifers." We are glad to learn that Mr. Bolton has been awarded a Gold Medal of the International Fisheries Exhibition for his exhibit of Invertebrata, and through his kindness we are enabled to give our readers a copy of the design of this medal.

Hulme Field Naturalists.—At the last ordinary meeting, Mr. Richard Astley read a paper on the Age of Trees. He explained the mode of formation of the annual rings, and stated that they were but a partial test of age because of the irregularity of their arrangement caused principally by heliotropism. He cited the case of a specimen now in the Kew Museum which exhibits 250 distinct rings on one side of the section and some fifty on the other. Again, we are met by another difficulty in this method of ascertaining age. The rings can only be counted when the tree is in its prime, as old trees are frequently hollow or such a deposition of lignine has taken place in the tissues as to obliterate the markings. The only reliable method is to measure the girth at a given height above the ground, comparing the results of a large number

of trees, and thus striking an average, which will give at least approximate ages. Trees measured in this way are found in many cases to be of less antiquity than was formerly supposed. The average age of oaks, for instance, is put down at 1,000, whilst this method reduces it more than one-half.

MARINE AQUARIA.—The usual ordinary weekly meeting of the Lower Mosley-street Natural History Society was held on Monday evening, Jan. 14th, when Mr. Robert Graham read a paper on the management of Marine Aquaria which was illustrated by practical demonstrations on a small aquarium brought for the purpose and containing a crab which had recently cast its exoskeleton, a mass of sea-weed, several small anemones, which were born in Mr. Graham's large aquarium at home, and a young star-fish, which was very active during the whole of the evening. Mr. Graham, who has kept marine aquaria for several years, gave some valuable information relative to their successful management, and dwelt at length upon such matters as the preparation of the bottom, stocking, feeding, filtration and aerifying of the water, and the removal of decaying matter. The demonstrations were the filtering of the water by means of cotton-wool, removal of matter from the bottom by a glass tube, and feeding of the anemones with small portions of mussels, which were conveyed to them by a pair of wooden forceps. It was noticed during the latter operation that the wily crab pounced upon one of the anemones and deprived it of its morsel. In referring to anemones it was stated by the lecturer that a species of Actinia had been kept in an aquarium for more than twenty years, and during that space of time had given birth to 324 young, 240 on one occasion being born in one night.

MANCHESTER MICROSCOPICAL SOCIETY.—*Annual Soiree.* The Annual Soiree of this Society was held in the Lecture Hall of the Athenæum on Friday, Jan. 25th, when Dr. H. C. Sorby, F.R.S., delivered a very important address upon "The Application of Quantitative Methods to the Study of the Free-Swimming Minute Animals in Fresh and Sea Water," showing the apparatus and methods used in collecting the material for examination—Small animals common in fresh water—Quantitative distribution of the minute animals in pure and impure rivers—Distribution in rivers, canals, ponds, &c.—Small animals, &c., common in sea water—Distribution in relation to the depth of the water and the state of the tide—Distribution in various localities at various times of the year—Connection between the number of the minute animals and the success of oyster culture, and also the distribution of certain common fishes—Drawings of the animals and plants, and tables of the results, shown as lantern slides, were shown with the oxy-hydrogen lamp.

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AND

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THE FORMS, ORIGIN, AND DEVELOPMENT OF THE TEETH.

By Parsons Shaw, D.D.S., (U.S.A.)

THE predecessors and the equivalents—the ancestral forms and the analogues—of the structures we know as the teeth, are found among the earliest products of the animal economy. As soon as animals begin to be predatory, they must have some means of capturing their prey. Hence we find hard organs of a chitinous, horny, or other substance at the extremities of the appendages and around the mouths of most creatures, which are ingeniously contrived for seizing, holding, and dividing the prey. It is a great step from a beetle to a tiger, but there is much similarity in the forms and uses of their claws; and when a chetah plants his canine teeth in the neck of an antelope and sucks its blood, he repeats what a water-scorpion does when it grasps its prey, and then, by means of its rostrum, feeds upon the juices. The first office, therefore, of these hard appendages is prehensile; and that office they retain, in a greater or less degree, to the end. In many of the shell fishes we find structures on their appendages identical in shape and use to those of insects. In the mollusca we begin to discover an approach to the arrangement we call teeth in the higher forms of life, for we find in the buccal masses of these animals what are termed odontophores. These are membranous plates, upon which are arranged vast numbers of minute cones and hooklets, forming a strap-like masticating organ. These odontophores are protruded from the mouth to scrape up the food. Those who keep aquariums will have noticed how soon a snail will clean off the confervæ from the glass. This is done by its odontophore. This structure is also sometimes used to remove the

* A paper read before the Manchester Microscopical Society.

carapace of another animal, so as to get at its soft parts for food. The variety and great beauty of these structures can only be appreciated by careful investigation under the microscope. The odontophores are probably chitinous in substance, although we might easily imagine them to be composed of true dentine, which they so much resemble.

From odontophores to the teeth of fishes is a long jump, but we still find the teeth of most fishes retain the same prehensile characteristics. In fishes we begin to discover distinct dental structures formed around the maxillary arches. It is true that in the lowest forms—such as the glutinous hag, and kindred species—the dental system is represented by only a single tooth; but as we progress upwards we find an almost endless variety of dental structures. So that, as Dr. Richard Owen says, "In form, substance, situation, and mode of attachment fishes offer more striking modifications than do those of any other class of animals; and the anatomist finds a difficulty in obtaining a command of language sufficiently varied to portray the singular diversity and beauty, and the interesting physiological relations, which are manifested in that part of their organization." But while there is such a variety in the teeth of fishes, a great many of them are in shape simple cones. The office of these cones is almost wholly prehensile, and they answer the purpose required so long as the animal lives on food that is swallowed whole. But when the food has to be divided into pieces, new forms of teeth are developed. First, we find the cones attaining great size, as in the canines of the carnivora, for the purpose of seizing the prey and tearing the flesh; and moreover these tusks eventually become weapons of attack and defence. The use of these teeth is apparent even in the less prominent but more complicated canines or eye-teeth of man, for we yet see the less refined classes gnaw a bone by holding it in the fingers instead of scraping it with a knife. Next we find the cones coalescing, so as to form the "sectorial" or scissor-like teeth of the carnivora. These teeth seem to be formed expressly for the division of the flesh. Then we find another combination of cones, to form what Dr. Owen calls the "tubercular molars," which are used to crack the bones to get at the marrow, as seen in the dog, hyæna, &c. The coalescence of the simple cones to form the teeth of the carnivorous and omnivorous animals must first commence in the union of two or more of the germs from which the teeth arise, although we are as yet unable to fully demonstrate this proposition. For we find the crowns of these teeth made up of a number of cones fused into one, over which the enamel has flowed, following the elevations and depressions caused by the points of the cones projecting beyond the general mass; and then, when the crown has formed, we find the fused tooth germ again partially separating to form the

roots of the human and other teeth. But this separation is by no means regular.

When the animal becomes herbivorous, a yet greater differentiation becomes necessary; for there must be large and flat teeth with rough surfaces to grind the food into fine particles. Therefore, a varied, involved, and complex dental system prevails among the hoofed quadrupeds. The specimens of the teeth of the elephant, cow, horse, rhinoceros, ibex, and wappiti deer we have here will give a good illustration of their general structure. The coalescence has in these teeth been carried very much further than before, and it has produced an entirely new combination in the hard parts of the tooth. We have, in the first place, the same kind of coalescence whereby several cones make up one tooth. But, in addition to that, we have several of these teeth, each with its own germ, uniting to form one huge grinder. Thus we see in this horse's tooth no less than five teeth united to make one. This union may be described as if my fingers and thumb should get joined together just where they meet, and then a substance should flow all through the interspaces and around the outside, just as the cementum (hereafter to be described) flows into all the interspaces of these five teeth, and all over the crown, forming a square tooth as in the horse. By this arrangement, instead of the enamel being on the outside, as in the human and many other teeth, it runs all through this complex structure. The effect of this arrangement is that the softer cementum gets worn away almost as soon as the tooth is erupted, and continues to wear down faster than the other structures; then the dentine wears away the next, and the enamel, as the hardest part, wears last, which always leaves on the grinding surface of the crown an uneven surface, particularly adapted for grinding the food. Look at the sharp projections in a deer or rhinoceros tooth. What cannot such an arrangement do in the way of mastication! The deer and the ibex have teeth made up of but four instead of five teeth, as in the horse.

Not only do we find these modifications in the arrangements of the hard parts of the teeth in the herbivora, but there are other most important changes. Among horses the canines, which are so large in the carnivora, are very small and useless in the stallion, and merely rudimentary in the mare. In the ox, sheep, antelope, &c., modification has gone still further, for these have neither canines nor incisors on the upper jaw; and the canines in the lower jaw, if indeed they can be called canines, are more like incisors. Where the upper incisors are absent, a rough pad is developed for the lower teeth to strike against, as may be seen in any cow's mouth. Instead of huge canines, claws, &c., the herbivora are provided with horns, antlers, &c., for defence; or when it is

only necessary to escape from an enemy, they have acquired fleetness or cunning. The appendages of insects and shell-fish are frequently cast off; and, as with the odontophores and the teeth of fishes, are being constantly renewed. But in the higher forms of animals there is generally but one renewal; and when the permanent teeth are once in place they must suffice. The elephant is a notable exception to this rule; for of the six huge molars he gets on each jaw there is never more than one, or at best two partially, in place and use on each side at a time. The series is continually in progress of formation and destruction, of shedding and replacement, the new tooth succeeding the departing one horizontally from behind forwards, none being displaced by vertical successors. Both male and female elephants have tusks. These are really the incisors, although in structure they are not quite like ordinary teeth, but consist of the modified dentine called ivory. This is recognised by striæ proceeding in the arc of a circle from the circumference in opposite directions, and forming by their decussations curvilinear lozenges, and very much resembling the engine-turning on a watch case.

The teeth are not bones. Bones are developed by the metamorphosis of true cartilage and ordinary connective tissue; and in both cases the cells are converted into lacunæ, or what Kölliker calls bone-cells; but the teeth are dermal structures. That structures do arise from the dermis is apparent in the hoofs, hairs, &c., of animals, the carapaces of some of the crustacea, the scales of fishes, and so forth. Indeed, according to Gegenbaur, the ancestry of the whole dental system can be traced back to the placoid scales of fishes; and this view has been adopted by the best authorities. Of these scales Gegenbaur says they have "the structure of dentine, are covered by enamel, and are continued into a plate formed of osseous tissue." Therefore, he calls them "dermal denticles." But how do structures that originate on the external surface for protection eventually develop on the internal surface for purposes connected with digestion, and assume such different forms? This is not difficult to understand when we remember, first, that the mucous membrane is only a differentiation of the skin; and, second, that constant variation to suit varying conditions is the order of life. As scales are formed on the skin of fishes we can see how easily they might extend beyond the border line which divides the external from the internal coating and spread over the mouth. Now, once begin the development of hard substances in the mouth, if we but allow the inconceivable ages which the comparative anatomist tells us have been required for the development of the higher animals, and the long succession of ever-changing environment which all animals are constantly passing through, it is not difficult to imagine how the primitive forms

became gradually modified from dermal scales into dermal cones, and then, starting from that simple form, became further modified in the way I have pointed out, so as to become adapted to the various kinds of work they have to perform. And in this way I think we can account for the different varieties of the teeth, while they retain some of their primitive characteristics.

The teeth are fixed in their position in the mouth by various methods, which pass by gradational forms into one another; so that a simple as well as absolutely correct classification of these arrangements is impossible. But we may reduce the modes to four general forms, and say it is either by means of attachment by fibrous membrane, or by a hinge-like arrangement, or by direct adhesion (or anchylosis) to the jaw, or by implantation in the bony sockets which arise from the maxillary bones for that express purpose. Most fishes, particularly the sharks and rays, afford an excellent example of the first-named method, whereby teeth are held solely by the tough mucous membrane which covers their more or less calcified jaws. In fishes of predatory habits there are found teeth which yield to pressure, and then, by means of a dense fibrous elastic ligament, radiating from the side of their base on the subjacent bone, return into their position with a snap. The use of these hinged teeth is to catch the prey, and also, as in the pike, for swallowing it when caught. Again, a tooth is so completely in unison with the adjacent bone that it is difficult to see with the unaided eye the line of juncture. Lastly, there is a special development of bony sockets, which arise from the maxillary arches and envelope the roots of the teeth, as in the human jaw. These sockets are perfectly subservient to and dependent upon the teeth, and soon become absorbed when the teeth are lost. In some cases the sockets are absorbed before the teeth are lost. There is no anchylosis, or fusion of the roots of the teeth to the bone; but between the roots and the "alveoli," as the teeth sockets are called, there is a highly organised membrane known as the "alveolar dental periosteum." This is the membrane which is severed when the tooth is extracted, and it also plays an important part in producing the pain from a devitalized or so-called "dead" tooth. The notion so much promulgated by the uninformed dentist, that a "dead" tooth will not be the cause of pain, is founded on a misapprehension of the facts of the case. This membrane serves as a cushion to the tooth, and is also its attachment to the socket by means of the prolongations of its vessels into the walls of the socket on the one side and the cementum on the other. This accounts for the necessity for a covering to the roots of the teeth which is not so dense as the dentine.

(To be continued.)

INFUSORIA FROM A WATER-BUTT.

ONE day having run out of Myriophyllum, &c., to examine under the microscope for my favourite pond life, I thought I would see if anything could be found in an old water-butt in my garden, the inside of which was coated with confervæ, and in this rather unlikely place I found a good stock of Saville Kent's beautiful Choano-Flagellata, besides other Infusoria. The principal species were *Codosiga botrytis*, a *Monosiga*, which I think is *gracilis*, although inhabiting fresh water; *M. brevipes*, *Salpingœca fusiformis*, *S. gracilis*, *Vaginicola crystallina*, and a few colonies of *Desmarella moniliformis* of Kent, or *Phalanx* of Stein. The latter is most beautiful under careful illumination, and decidedly rare, as I only found about a dozen in some month's search, as after my first success I made several raids upon the water butt. I should like to know from your readers if *D. moniliformis* is a rarity, as a friend who has been working at the Choano-Flagellata for some years had never seen this species before I gave him some of this confervæ. I also found a few very curious monads that may be a distinct species, but more likely only a variety of *C. botrytis*. In each case there were two Zooids joined like this—

They seemed to be joined together by a kind of membrane—the substance of which seemed to be similar to the collar. I had one pair under examination for nearly twelve hours with an $\frac{1}{8}$ Zeiss oil immersion, and the illumination very carefully attended to, so I think there was no mistake about it, particularly as I have examined hundreds of *C. botrytis*, and had never seen this structure before, and have only seen two or three like it since. I should much like to know if this has ever been noticed before. I find that the very best objectives to exhibit the collar well (which is not an easy thing to show properly) are oil immersions, and with the light taken direct from the lamp, as Kent recommends, and the flame focussed carefully on the monad. A friend of mine, who believes in dry lenses with narrow angles for this kind of work, was exhibiting at my house one night a specimen of *C. botrytis*, with a dry $\frac{1}{8}$ of 110°, and I asked him to allow me to put on a homogeneous $\frac{1}{8}$ by Zeiss, and he was quite struck by the difference of the picture, the oil immersion showing the collar magnificently. It is true these lenses are a little more trouble to use, but if they show these minute Infusoria so much better than dry ones the trouble ought not to be counted. I have the $\frac{1}{8}$ and $\frac{1}{12}$ of Zeiss in constant use, and hope before long to possess a $\frac{1}{25}$ of 1.38 N.A. by Powell and Lealand for the same work.

C. L.

EXTRACTS FROM MR. H. E. FRIPP'S TRANSLATION OF PROFESSOR ABBE'S PAPER ON THE MICROSCOPE.

Monthly Microscopical Journal, vol. xiv.

A CAREFUL consideration of the means at the disposition of the optician, and a critical comparison of the difficulties serving as a guide to the discussion of the conditions influencing them have led me to the conclusion that lenses and systems of lenses of which each part has prescribed dimensions, can be executed with an exactitude that fairly ensures correct action, and with greater facility than any other mode of procedure offers for the fulfilment of the same conditions with equally good results.

In the workshops of C. Zeiss, of Jena,* the construction of objectives, from lowest to highest power, is regulated by strict calculation for each single part, each curve, each thickness of glass, each degree of aperture; so that all guesswork and "rule of thumb" is avoided. The optical constants of each piece of glass are obtained from trial-prisms by means of the spectrometer. Each constituent lens is ground as nearly as possible to its prescribed dimensions and accurately fitted. In the highest-power objectives only is the lens distance left variable, in order that slight deviations from accuracy may be adjusted. And thus it has been shown beyond dispute that a well-grounded theory, combined with rational technical processes, may be successfully substituted for empirical practice in the construction of the microscope.

The fact that an amount of angular aperture, which is unknown in any other instrument, comes here into question, renders the accepted ideas of "aberration" entirely useless, and the result of investigations which were undertaken in order to bring the question to some issue, was the discovery that an important feature in the optical functions of the microscope had been hitherto overlooked. In all previous explanations or interpretations it has been accepted as a self-understood proposition that the formation of an image of an object in the microscope takes place in every particular, according to the same dioptric laws by which images are formed in the telescope, or in the camera; and it was, therefore, tacitly premised that every function of the microscope was determined by the geometrically traceable relations of the refracted rays of light. A rigorous examination of the experiences upon which the traditional distinction of "defining" and "resolving" powers is founded, has shown that the proposition is not admissible. It holds good, in-

* And we may add, in all the well-regulated workshops in this country.—ED.

deed, for certain cases, capable of definite verification, but for the generality of objects, and particularly for those objects on which the microscope is supposed to exhibit its highest quality of performance, it appears that the production of microscopic images is closely connected with a peculiar and hitherto neglected physical process, which has its seat in and depends on the nature of the *object* itself, although the measure of its effect stands in direct dependence upon the construction of the *objective.* It is hence possible not only to fix the limits of the visible, beyond which no further resolution of structure could be expected, but also to bring to light the fact that a microscopic image which may be entirely free from error in itself, and therefore be supposed to represent in all cases the true structure of an object, nevertheless does *not* do so for a whole class of objects and observations.

In addition to those images *of the object* which are thrown off by the lenses of the microscope, a series of *associated images of the aperture* are simultaneously thrown off, which together form an image of the outwardly projected plane of aperture. This latter (aperture image) is thus associated with the final virtual image of the object, and appears at the eye-point, so called, above the ocular where it may be examined with a lens. But the image of the object, so far as it is produced by the objective alone, lies in or close to the upper focal plane of the objective, where also it may be seen by looking down the tube of the microscope with the naked eye. These two sets of images are interconnected by common relations, the determination of which affords a key to the solution of questions scarcely to be approached by any other means. All the characteristics of the *object images* hang together with certain characteristics of the *aperture images*, and *vice versâ.*

The principle on which is founded the study of these aperture images leads to various results, depending for their full development upon a principle which constitutes at the same time a law of fruitful application throughout the whole theory of the microscope, and which may be thus formularized.

When an objective is perfectly aplanatic for one of its focal planes, every ray proceeding from this focus strikes the plane of the conjugate focus at a point, whose lineal distance from the axis is equal to the sum of the equivalent focal length of the objective × the sum of the angle which that ray forms with the axis.

Now as this condition must be fulfilled in every correct instrument, both for the objective and for the whole optical part of the microscope, the formula above given establishes a relation of quantity between the *angle of aperture* of the microscope and the lineal diameter of the aperture images above the objective and ocular.

Moreover, it is thus possible to determine, by micrometric measurement of the position in the upper focal plane of the objective

which the track of any ray occupies, the direction which it took before entering the microscope. Consequently the aperture images formed above the objective, when examined with a suitable micrometer eye-piece, can be used for measurement of the divergence which the rays coming from the object undergo.

In the next place, we need a more characteristic exposition of the optical functions which, in the case of images formed under larger angles, by rays having a *great* inclination to the axis, differ greatly from the abstraction by which theory represents the action of a set of lenses in forming an image. And such an exposition offers itself when we can define by axioms of general validity the mode in which an image is focused and spread out on the focal plane of an optical system, and distinguish the *focusing function* and the *extension of image* over a surface as the two principal factors of the image-forming process, alike independent in their abstract idea, and distinct in actual specific function. Apart from the fact that no exhaustive analysis of a faulty image nor any means of perfect correction are possible until such characteristic distinction can be laid down, we have no other means of determining the part taken by each constituent element of a compound system of lenses in the joint performance of the whole. When then we define the function of the objective to be the production of a real image, and the function of the eye-piece the amplification of this image,—such explanation does not by any means reach the essential principle of action of the compound microscope. This is obvious at once when we consider that by such a definition the combination of objective and eye-piece is made only to indicate *magnifying power*, whereas on the contrary the remarkable superiority of compound over simple microscope consists in the *quality of its performance.* By the *objective* an image is formed and spread out in what is an almost perfect accordance with the laws by which images of infinitely small elements of a surface are formed. By the *eye-piece* a displacement of focus is effected; that is to say, a change of divergence of each separate pencil of light takes place till the divergence is almost imperceptible, and the pencils infinitely fine.

The first step or act in the image-forming process consists, not in the production of a reversed image by the objective in front of or within the ocular, but rather in the production of a "virtual" image at an infinite distance with parallel rays. The *second* act comprises the last refraction through the posterior surface of the objective, and the several refractions taking place in the ocular by which the image is re-formed at the distance of clear vision with diverging visual angles. The first act answers plainly to the function of an ordinary "magnifying glass"; while the second, taking all the changes comprised therein together, answers as obvi-

ously to the functions of the telescope (possessing only a small objective aperture) to which the virtual image formed by the first process serves as an "object."

This interlocking of objective and ocular functions—presenting the combined effect of a magnifying glass and that of a telescope—must be laid down as the most general and correct characteristic of the principle upon which the compound microscope of the present day is constructed.

From the foregoing remarks may be gathered a theory of aberrations, sound and strong enough to master the difficulties which the application of exceptionally large angles of aperture to microscope objectives has occasioned.

It appears that the faults of image formation are separable into two distinct classes, one comprising faults of the focusing act (aberrations in the strictest sense), the other comprising faults of the amplifying function. To the first class belong those spherical and chromatic aberrations commonly studied; in the second class must be placed a series of peculiar deviations of rays of light from their normal course, which arise from the circumstance that the separate rays of a homofocal beam occupying the aperture of the lens yield unequally magnified images, according as their inclination to the axis varies, and according also to the unequal refrangibility of the different colours—an inequality which obtains just as much whether the several partial images are compared with each other, or whether within the area of each image different positions in the field of vision are compared.

This class of anomalies affects exclusively the constitution of the image outside the centre of the field. The perfection with which the rays unite in the central region, and therewith the maximum capacity of performance, depends on the contrary entirely on the real aberration spherical and chromatic, as commonly understood.

Chromatic aberrations, as they show themselves where a large angular aperture is used, do not depend alone on those differences of focus which affect the image-forming beams as a whole; but quite as much in an unavoidable inequality of coincidence of colours of variously inclined pencils of rays within the angle of aperture, which manifests itself in this, that an objective which is perfectly achromatic when direct illumination is used must be more or less *over*-corrected for use with oblique illumination. Although the first-mentioned ordinary form of colour dispersion (primary and secondary) may be entirely removed or rendered scarcely noticeable, the last-named source of chromatism cannot be counteracted or removed by any known material or any known technical treatment.

Spherical aberration on a stricter examination of its causes re-

solves itself into a series of independent elements which as they increase in number, follow, with the increasing inclination of the rays towards the axis, a more and more unequal course. An absolute effacement is only possible theoretically for the two first members of the series. As soon as the angular aperture exceeds a small number of degrees, the counteraction of spherical aberration can be effected in no other manner than by compensating the irremovable errors of the higher elements through intentionally introduced residual aberrations of the lower ones. The accumulation of unavoidable deficits which this method of compensation necessarily leaves unremedied, compels a limitation of the angle of aperture. For angles of aperture exceeding 60° and a *fortiori* for the very large angles of modern objectives, the pre-supposition of an adequate compensation is found in the well-known type of construction where a plain nearly hemispherical front lens is combined with a strongly *over*-corrected system of lenses. The discovery of this mode of construction must be looked upon as the basis of every improvement which has been introduced since. For a system of lenses made to use in air, the limit of serviceable aperture proves to be from 105° to 110°, beyond which it is not possible to counteract sufficiently the spherical aberration, except by lessening the focal distance of front lens from the object to a degree which makes it practically useless. The application of the immersion principle renders it possible to overcome spherical aberration, where even the *maximum* angular aperture is used. It is in this power of using very large angles of aperture, and also in avoiding loss of light, that the real advantage of the immersion plan lies. It will indeed be seen from what follows, that these two facts fully explain the undoubted superiority of the immersion lens.

Every appliance by which the amending of spherical aberration has been attempted—whether by correcting lenses placed above the objective or by construction of ocular—will produce no better result than what is already effected by changing the distance of the front lens of the objective from those behind it. They simply permit the existing residual aberration to be transferred—shifted backward or forward between the centre and outside border of the aperture—and by this means to keep, for a time, some particular zone of the objective more or less free from aberration, *at the cost of the rest!*

In an analysis of the conditions which belong to a perfect construction, it becomes obvious that the factors on which correctness of image in the centre of the field, and the maximum of good performance depend, namely, chromatic and spherical aberration, pertain to the functions of the *objective* alone, upon which no influence of the eye-piece, however constructed, can produce any

marked effect. Arguments advanced in favour of a long tube or of a short tube are untenable in theory; and the supposed differences of effect have no real existence when examined under conditions which are truly comparable. There will be found in every objective a particular angular amplification obtainable at will by means of length of tube and strength of ocular, which must exactly suffice to enable any eye possessing normal capacity of vision to recognize all the details that can possibly be delineated in the virtual image formed by the objective. And this, which may be termed "necessary *angular* amplification," may be looked upon as the measure of the relative perfection of the objective.

(To be continued.)

NEW METHOD OF DETECTING TRICHINÆ IN MEAT.

SLICES, two or three millimetres in thickness, are taken from several different parts of the meat to be examined. The pieces are preferably taken from the surface of the muscular portion of the meat. A series of thin sections are made of each of the pieces, and these are all plunged into a solution composed of methyl green 1 gramme, distilled water 30 grammes. After about ten minutes' maceration the sections are taken out, and placed to decolorize in a large vessel filled with distilled water. They remain there about half an hour, the water being agitated and changed two or three times. Finally, the water having become quite limpid, it is stirred up with a glass rod, interposing the vessel between the eye and the light, when the sections containing the trichinæ are distinguished quite readily with the naked eye. The trichinæ appear in the form of small, elongated particles, of a fine blue colour. The methyl green becomes fixed to the cysts of the trichinæ with greater tenacity than to the other parts of the tissue.

It suffices then to examine the sections with a magnification of fifty diameters to distinguish the worm which will be found enclosed in the cyst.

If, in following this method, no trichinæ are found, it is positive assurance that the meat is not infested with them.—*Bull. de la Soc. Belge de Micr.*

M. PASTEUR'S RESEARCHES.

M. PASTEUR, in his own name and that of his assistants, MM. Chamberlan and Roux, made a communication recently to the French Academies of Sciences and Medicine on the results of his experimental inoculations with the virus of rabies. He finds that the virus may remain in the nervous tissues without manifestation for three weeks, even during the summer months. Virulence is manifested not merely in the nervous tissues, but in the parotid and sub-lingual glands. The granulations observed in the fourth ventricle, when in a state of virulence, are finer than the granulations in the fourth ventricle when in a healthy state, and they can be coloured by means of aniline derivatives. The virus of rabies injected into the veins or beneath the skin produces paralytic rabies, while inoculations into the spinal cord or the brain produce the paroxysmal form. Inoculations with quantities of the virus too small to be effective, have no preservative influence against subsequent inoculations. Whether the virus is propagated by means of the nervous tissues, or by absorption through the surfaces of the wound has not been ascertained. Finally, the experiments have shown that the protective "attenuation" of the virus is possible. The energy or the nature of the virus varies in each species of animals. By passing the virus through different animals "cultures" are obtained whose precise effects can be predicted. Thus a "culture" has been obtained which certainly kills the rabbit in five or six days, and another which certainly kills the guinea-pig in the same time. Other things being equal the virulence varies inversely with the duration of the incubation. M. Pasteur and his assistants have good reason to believe that by means of a special culture they have succeeded in making twenty dogs absolutely proof against rabid inoculations. M. Pasteur, with his usual caution, asks for a little longer time before finally pronouncing on the condition of the dogs in question. To devise a means of making the dog proof against rabies is of course to devise a means of almost certainly preserving man (including children) from this frightful disorder; for it is almost invariably communicated to man and other animals by the bites of rabid dogs.

FRESH WATER SPONGES IN THE FOREBAY AT FAIRMOUNT.

THE inlet by which the sweet waters of the placid Schuylkill are conveyed to the turbines of Fairmount is undergoing renovation. A peep into its muddy depths shows the walls to be covered with a brownish layer, while a double line of broken iron pipes is coated with what appears to be rust. This glimpse is enough for ordinary mortals, but did not satisfy the mind of Mr. Edward Potts, our Philadelphia spongiologist. With the binocular intention of finding out something new about sponges, and of ascertaining whether his humble pets were responsible for any considerable proportion of the impurities in our drinking water, Mr. Potts descended into this Avernus. The results obtained, omitting the details of the trip, were given at the meeting of the Academy of Natural Sciences on Tuesday last.

Nine-tenths of the incrustation, which covers the walls to a thickness of three-eighths to one-half an inch, consists of the spicules, or solid needles of silica, and of the statoblasts, or winter nests of a small sponge known as *Meyenia Leidyii.* With this is a much smaller quantity of the larger and somewhat branching *Spongilla fragilis.* The material upon the iron pipes is also a layer of the former sponge. The speaker not only denied that sponges added much to the impurities of the water, but stated his belief that those living in fresh water did not, under ordinary conditions, *die at all.* This may seem a wild idea to those who think that all living things must ultimately turn to dead bodies; but the fact is, that it is well-known that all those forms of life which consist of a single cell (protozoa) do not die, as higher animals die. They increase by division, then divide again, and so on, till ultimately they become quiescent, surround themselves with a horny pellicle, and thus rest awhile. When the proper conditions surround the little cyst or sac, the covering bursts, and hundreds of tiny spores flow out, each of which, after growing awhile, divides and subdivides like its predecessors. Thus the protozoa leave no dead bodies. The sponges are essentially large aggregations of one-celled animals, living upon a more or less thoroughly woven skeleton, formed of needles of flint (silica) or lime, most commonly the former. It has long been known that in winter the sponges which live in fresh water form winter eggs, or rather winter nests (statoblasts), and it has been usually thought that the greater part of their soft animal substance, or sarcode, slipped off into the water and there decomposed. This Mr. Potts believes not to be the case. He believes that the living cells are all drawn together and concentrated in the statoblasts, just as protozoan in its cyst. He has seen the living particles of the crea-

ture moving slowly over the structure of the sponge, and has noted that when the old skeleton remains intact, the living matter that issues in the spring from the openings at the top of the statoblasts will reclothe their former framework. If it is washed away, the cells secrete new spicules, as those of our body secrete new bone.—*Philadelphia Record.*

BOLTON MICROSCOPICAL SOCIETY.

AT the March meeting of the Bolton Microscopical Society an interesting paper was given by Mr. Mawson Harvard on the "Spore Cases of Ferns," of which the following is a condensed report :—

Spore cases, which are the provision that nature has made for the production, development, preservation, and diffusion of the spores, are sometimes called *sporangia*, *thecæ*, or *capsules*, and are collected into groups of varied form, called *sori.*

The forms assumed by these sori are usually distinct spots, round or oblong, or lines, more or less extended. The principal ones being the *punctiform*, the *oblong* or *linear*, the *amorphous*, and the *universal.*

In most ferns the sori are borne on the under surface of the frond, and are then said to be "dorsal." In others they are protruded from the edge of the frond, and are called *extra marginal;* these are often collected round the free extremities of the veins, which are surrounded by thin urn-like expansions of the cellular tissue. But there are some curious exceptions to these usual modes of development.

The point at which the sorus is fixed to the frond is called the *receptacle.* This receptacle is formed by an expansion of the tissue, at some fixed point of the venation, sufficiently constant to acquire systematic importance from the fact.

The spore cases of the greater number of known species of ferns are small, roundish, hollow, one-celled bodies, nearly surrounded by an elastic belt, which ring is called the *annulus*, and these ferns are called *annulate* ferns. In a few classes the spore cases are without any trace of ring or annulus, and are called *exannulate* ferns.

The spore-case itself consists of a thin cellular shell, without internal divisions, traversed externally by a single line of short transverse parallel thickened cells, which form the belt or ring. It seems to be the elasticity of this belt or ring, which, in some species of ferns, takes a vertical direction, and in others a horizontal one, that causes the case to burst, and liberate the spores.

In certain annulate groups of ferns the spore cases spring from

the receptacle without any perceptible covering, and are called *naked*, whilst in others they are covered while young with a membraneous cover, called an *indusium*, which bursts as the spore cases grow, and is either cast off or pushed aside. When a similar membrane is placed beneath the sorus it is called an *involucre*, but this is seldom met with.

These indusia, which are either *special*, *accessory*, or *universal*, take various and beautiful forms.

After Mr. Harvard had concluded his paper the President of the Society, Mr. C. L. Jackson, F.R.M.S., F.C.S., F.L.S., gave an interesting lecture on "Fish Scales," illustrated by the oxy-hydrogen light.

The lecturer drew attention in the first place to the difference between the scales of reptiles and fishes, the former being merely folds of the skin, while the latter are bony, or bony plates embedded more or less in the skin. A large number of sketches of the forms of fish scales were thrown on the screen by the oxyhydrogen lantern, and the different varieties were described, notice was particularly given to the curious fact that in geological times the scales of the fishes, which then existed, were massive armour plates strongly interlaced, or even dovetailed together. We have at present very few species of fish carrying such a covering. The variations in the form of scales under the influence of artificial culture was illustrated, and at the close of the lecture there was an interesting discussion on the use of scales to the fish, and great admiration was expressed at the wonderful beauty, and great variety of their forms. A large number of specimens were exhibited under the microscopes, and at the close of the lecture Mr. Jackson exhibited some very beautiful specimens of embroidery in fish scales. The lecturer concluded, after acknowledging the kindness of Mr. Sachs, of London, in assisting him to procure specimens, &c., and of Mr. Pennington and Mr. Russell, in assisting him with the preparation of the lantern slides and the manipulation of the lantern.

DIVISION OF LABOUR AMONG MICROSCOPISTS.

By Prof. J. M. Mansfield.

A paper read before the American Society of Microscopists, Chicago Meeting.

THERE is now no systematic, conscious oversight in the multitude of researches in microscopy. Each man, moved by the impulse of discovery, pushes out in whatever direction his immediate surroundings or accident may suggest. He could often just as

easily think in some other department of the subject. One way to account for the great number of scientific papers in Germany is that young Germans are taught in the universities to look about for some unexplored thought and to work it as exhaustively as possible for publication. And while German microscopists are not organized for the systematic movement meant in this paper, yet they do the next thing to it—that is, push out and work out some department of thought, though it is done without oversight. Now, what we want, in the whole domain of microscopy, is that same oversight in all the work that a great business manufacturer has over each man in his employment. If he has a thousand hands, each one has his work assigned him. I think if each hand should go, without the guiding reason of the controlling mind, and find by accident his work, or turned to whatever happened to be near him, it would be something like the present state of our science. Some of the hands would by their very genius do good work in such bad government, but the most of them would neither find their places, nor work enough to bring up their part of the manufacturing, and hence the work as a whole would suffer irrevocable loss.

The short of the matter is, we must remember Bacon's urgent directions for division of labour, which necessitates a committee to divide up the whole science and its applications, choose men for each party, aud as there are not enough living microscopists to take all the pieces, as mountains of microscopic truth will be untouched after every living microscopist has added his wreath to the science, they can look about for young people and others who will come and labour in this field. The new microscopists coming to work will gladly prepare for the subject which the committee of oversight has assigned them. Thousands of men will go at once to assigned work who will never go if left to themselves. It is difficult for one just beginning a science to know what to do, or what parts are already worked up. Many away from good microscopic libraries of reference can not know what is now well done, and will not undertake a research which they think may be already done. Under this plan of conscious oversight one starting could be easily informed by letter from the committee what his part is, and be advised, if a new hand, of methods, works of reference, and other persons who could work with him; also the character of the publication, such as abundant illustrations, giving all the species in systematic botany, zoology, and mineralogy, photography of drawings, coloured or not, whatever is best; so in histology, and all branches of the subject. By an organized effort much might be done to lay down general methods of work to which all ought to attain. There is nothing in this to take any man from his present delightful study, but at once, all along the line, systematize the work to conquer the world. If some man is a genius called to a

special work, and can not work in other fields, either from his nature or his surroundings, let him push up a round higher, if he can, to what his heart and mind cling to. No one need close his eye to some other's truth. If such a union of work be undertaken by this society, in ten years it will have more effective systematic works than all the world of microscopists beside them. Vast volumes telling everything that can be known or needed now by so many studying in private. They are a necessity in laboratories. These volumes ought to be written from a great number of standpoints. They must be written to economise the labour of teachers and students. It is now well established that the advancement of such a science as microscopy must be pushed forward by experiment in the progress of the world. And this society knows the same is true of each individual who seeks entrance at these beautiful gates of this infinite, varied, and sublime panorama of microscopic truth. To give the student the best results he ought to have these exhaustive guide-books in his delightsome journey. For every thing on this planet must pass in the microscopist's field of view.

THE ABBE ILLUMINATOR.

MR. J. GRUNOW, of New York, gives the following instructions for using this illuminator as constructed by him :—

The apparatus consists of a lens-system of very wide angular aperture, two revolving diaphragm-plates, in conjunction with the plane and concave mirrors on the stand proper. The upper plane side of the lens-system should be almost even with the upper surface of the stage, so that it almost comes in contact with the slide. For observation by central light the diaphragm with central openings is used, viz., a narrower or wider diaphragm, according to the focal distance of the objective in use, the nature of the object-slide, and the intensity of the source of light. Generally, the narrowest diaphragm is to be recommended, as it gives sufficient light. Used without a diaphragm the condenser invariably gives an unsatisfactory illumination.

By moving the diaphragm openings to the right or left, partly out of the optical axis, oblique illumination is obtained.

For dark field illumination the star-shaped diaphragms are used instead of the aperture for central illumination, and always used in the central position. At the same time it is, however, preferable to reduce the aperture of all the high-power objectives, say from

one-fourth inch up, by placing a diaphragm in the back of the objective employed.* The diaphragm is, however, to be taken out again in every case when the objective is used for transmitted light. Objects not transparent cannot be viewed by this illumination, as the working rays of light have to pass through.

The polariscope can be used in connection with this apparatus. For this purpose the condenser must have room enough underneath the stage to have an attachment for holding the polarizer. Polarized light can be used then for central as well as oblique illumination.

In using the condenser, the plane mirror is generally used. Only when viewing with very low powers, when the plane mirror does not completely illuminate the whole field of view, the concave mirror is used. In every instance where the mirror is once adjusted for full illumination, the changing of the diaphragms does not affect it.

When using lamp-light, it is recommended to use as large a condensing lens as possible, or perhaps a large glass ball filled with water, in order to secure an evenly illuminated field of view without moving the flame too near the microscope. The condensing lens or the glass ball is placed in such a position between the lamp and the microscope that an image of the flame is projected on the plane mirror.

When, in using immersion lenses, very oblique illumination is desired, or when dark field illumination under high amplification is used, it is advantageous to place a drop of water on the upper surface of the condensing lens of the apparatus, so as to fill up the space between it and the under side of the object-slide with a medium denser than air.

The usefulness of this apparatus has been recognised by all who have become familiar with its use, and it is not only employed as an ordinary accessory, occasionally, but as a constant auxiliary in daily application.—*American Monthly Microscopical Journal.*

THE GERMAN CHOLERA COMMISSION.

A SECOND report from Dr. Koch, as head of the German Cholera Commission, has been published in the German papers. Dr. Koch states that, subsequent to the issue of his last report, several *post-mortem* examinations of cholera subjects were

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* The aperture shutter would be useful here.—Ed.

made by the Commission before leaving Egypt for India, the peculiar bacillus referred to in the last report being found in all cases. All attempts to communicate the disease to animals failed, however. Amongst the methods adopted in order to develop the virus were mixing the cholera matter with earth, and also with water, Pettenkofer's hypothesis that the germ arrives at the epidemic stage in the ground, and the inferences from the frequency with which persons who have washed the linen of patients have been attacked, being thus tested. Damietta was visited, and careful inquiries made with a view to ascertaining the facts regarding the probability of the importation of the epidemic from India. Dr. Koch promises to forward later on a detailed report on the results of this inquiry. On October 16, the Commission travelled from Alexandria to Cairo, in order to proceed thence to Suez *en route* for India. Before leaving Alexandria the quarantine stations near that port and Damietta had been visited, but the subsequent outbreak of cholera at Mecca led Dr. Koch to conclude that a visit to the Red Sea quarantine station was also extremely desirable. The members of the Commission were therefore conveyed from Suez to Tor and El Wedj, whence they intended to proceed to Jeddah to join the Indian steamer. The members of the Commission would, however, have been themselves compelled to undergo a long quarantine in Jeddah. In order to avoid this loss of time they returned from El Wedj to Suez, and joined the Indian steamer at that port on November 7. The visits to the Red Sea quarantine stations proved, Dr. Koch says, extremely instructive. He mentioned that on the day of their arrival at Tor 500 pilgrims were landed from a vessel of the Austro-Hungarian Lloyd's. The medical officer reported all well, but as the pilgrims landed the Commission observed several who appeared very ill, and detected unquestionable symptoms of cholera. A second vessel arrived during the visit of the Commission, and landed pilgrims. Cholera broke out amongst both parties, three deaths occurring amongst those landed from the first vessel, and one amongst those landed from the second, besides many cases of sickness, all under the eyes of the Commission. Dr. Koch promises a detailed report on the importance of the Red Sea quarantine stations, and on the circumstances alluded to, "which have so serious a bearing upon the transmission of the disease to Europe." The Commission, in the course of their inquiries, have also given attention to meteorological conditions, the influence of the rise and fall of the Nile on the course of the epidemic, drainage, water supply, and so on. During their stay in Alexandria the Commission also made some important observations on dysentery, the occurrence of tuberculosis in Egypt, and on the *Filaria sanguinis hominis* and other blood parasites. Dr. Koch speaks in the highest terms of the attention and un-

sparing assistance given to the Commission by the Egyptian Government. The decision to proceed to Calcutta instead of Bombay was taken because the Commission learned that the cholera epidemic in the latter port had ceased.

SOME THOUGHTS ABOUT MOUNTING.

By C. Henry Kain.

THE attention of working microscopists has been more earnestly directed of late to the matter of mounting media than ever before, and it is becoming more and more manifest that we have much yet to learn in this line. For many years microscopists were content to embalm everything in balsam; but such is no longer the case, for while it is still one of the most reliable mediums for many purposes, yet the microscopical student is learning to select a medium with special reference to the characteristics of the object to be displayed. Those who have examined the recent slides of diatoms mounted in phosphorus by Möller, or those mounted by Prof. H. L. Smith in his new medium, cannot fail to appreciate the fact that the selection of a proper medium is an important consideration as regards the revealing of structure.

The writer has two specimens of *Isthmia enervis* mounted while fresh, *i. e.*, without being cleaned in acids—one in Deane's medium and the other in balsam. The first exhibits finely the endochrome and membranous envelope of the diatom, but the silicious frustule is scarcely visible. In the second the envelope is absolutely invisible and only the silicious frustule can be seen.

Mr. Stephenson has shown that this revealing of structure is due to the use of a medium having a refractive index different from that of the object, and that the measure of visibility of minute structures is equal to the difference between the refractive indices of the object and the medium in which it is mounted. This being the case, it becomes a matter of serious moment to determine what medium is best adapted to a specific purpose; for not only must we keep in mind the proper displaying of structure, but also the fact that it is almost if not quite as important to preserve unchanged the characteristics of an object. One is likely to be suspicious of new methods, too; for it is not gratifying to a microscopist to look over his cabinet and find that mounts prepared in a certain way have become valueless in a few months. Hence, for valuable

material we must still adhere to the tried and reliable methods of the past at the same time that we are reaching out by experiment to learn what is better, for it is only after the lapse of time that we are able to pronounce accurately in regard to the success of a particular method.

Among the new media, the solution of biniodide of mercury and iodide of potassium is one that is likely—on account of its high index of refraction (1.68)—to prove very useful for many purposes. Nevertheless, it will be found to act injuriously upon some structures, and hence must be used with caution. It is undoubtedly superior to mono-bromide of napthaline for the mounting of diatoms, besides being much more pleasant to manipulate, for the mono-bromide is certainly the most frightful combination of smells that the art of the chemist could concoct. Some experiments by Mr. E. E. Read, of the Camden Microscopical Society, would seem to indicate that cosmoline may prove a valuable medium in which to mount the starches. The starch grains are certainly remarkably well displayed in it. How permanent the mounts may prove is a question of time. It is not improbable that several of the petroleum products—even the plebeian kerosene itself—may be found not unworthy of the microscopist's attention. Many of the vegetable oils and balsams, too, will doubtless be found useful, as some of them possess high refractive indices and at the same time are good preservatives. Oil of anise, according to Davis,* has a refractive index of 1.811, and the writer has used it to some extent with apparent success. Prof. Christopher Johnston has shown (*Am. Monthly Mic. Journal*, Oct., '83) that balsam copaiba and damar is likely to prove a valuable medium. Dr. W. W. Munson some time ago called attention to the preservative properties of a solution of hydrate of chloral, and the medium is evidently deserving of more attention than it has had. A slide of algæ put up in this solution over four years ago still remains as bright and pure as when first mounted, and, what is quite important, the cell contents of the algæ appear to be less contracted than is usually the case.

A compilation of the various formulas for mounting media which have been published from time to time would prove of great value, especially if accompanied by notes indicating the adaptation of each.

Next to the choice of a medium, and of almost if not quite equal importance, is the choice of a suitable cell (if one is used), and a proper cement. It is absolutely essential in the case of both that they shall neither be dissolved nor acted upon chemically by the mounting medium. It may not be out of place here to mention an amusing personal experience, especially as it points a moral.

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* Practical Microscopy, p. 98.

Soon after the publication of the formula for the biniodide of mercury solution previously referred to, I mounted a slide of *Amphipleura* in it, using a tin-foil cell. The next evening I took it out to exhibit to a friend, but was amazed to find the cell empty. A little investigation revealed the fact that the mercury in the solution had formed an amalgam with the tin-foil, completely destroying the cell. It was rather a mortifying blunder, that even a tyro in chemistry would scarcely have made. Shellac, rubber cement, Brunswick black, marine glue, gold size, etc., all have their appropriate uses, and the microscopist's best judgment should be exercised in making a selection. For use with an oily medium, the writer has found stratena to be well adapted, as it is not acted upon by oils. It works very smoothly, too, with a brush, but, as it is affected by water—and hence by the moistening of a slide, which often occurs —it should be covered with some other cement for a permanent finish.

As a transparent finish for ringing slides, I know of nothing better than either Brown's rubber cement or Queen's colourless marine glue, both of which work easily and dry rapidly. The latter will probably prove of much value as a cement in consequence of its insolubility in most mediums. My attention was called to this in trying to clean brushes with which it had been used. Alcohol and even benzole were ineffectual, fusel oil being the only true solvent.—*The Microscopical Bulletin.*

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Tubbs, Brook, and Chrystal, 11, Market-street, Manchester.

All matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, The Willows, Fallowfield, Manchester.

Death of Mr. Charles Stodder.—To Mr. Charles H. Bassett we are indebted for information in regard to the death of Mr. Stodder. The Boston *Transcript* of January 15th contained the following notice:—

"Mr. Charles Stodder, well known in this city, and formerly connected with the trade in optical instruments, died on Sunday at the age of seventy-seven. He was at one time in business in

the Rialto building. His funeral is to be held at Dr. Hale's church at 12-30 to-morrow afternoon." (Our informant is confident that his age was seventy-five, not seventy-seven, as above stated.—Ed.)

Mr. Stodder was an occasional contributor to the journals, writing upon stands and objectives, and upon the *Diatomaceæ*, of which he had a considerable knowledge. He was well known as business agent for Mr. Tolles, of whose death he kindly sent us information only a few weeks before his own.—*The Microscopical Bulletin.*

The Late Mr. J. H. Dallmeyer.—The death is announced in his fifty-third year of Mr. John Henry Dallmeyer, optician, whose name has been long associated with the production of some of the best optical work in England, especially lenses for different branches of photography. In 1849 he left Germany, his native country, and came to England, entering the house of the late Andrew Ross, the founder of the well-known optician's business bearing his name. Mr. Dallmeyer's attention was at first devoted principally to the construction of telescopes. * * * He was specially commissioned to provide several of the telescopes and photographic appliances used by the different Government expeditions for the observation of the recent transit of Venus, and his telescope object-glasses are in high repute among the leading astronomers. For some time past his health has been precarious, and while taking complete repose from work, on the voyage to New Zealand, he died on the 30th of December.—*English Mechanic.*

Anthrax.—An inquest recently held at Guy's Hospital (says the *Lancet)* serves again to direct attention to the fact which the wards of that institution have shown often of late years, that anthrax (wool-sorters' disease) occurs occasionally among the tan-yards of Bermondsey. The subject of the inquiry was a tanner.

The Late Professor Rolleston.—The Clarendon Press, says the *Athenæum*, will shortly publish in two volumes octavo "Memoirs, Addresses, and Fragments," of the late Professor Rolleston, arranged and edited by Professor W. Turner, with a biographical memoir by Dr. E. B. Tylor. These volumes contain a selection of the most important essays contributed by Professor Rolleston to the *Transactions* of various learned societies and to scientific journals, together with several addresses delivered before the British Association and other learned bodies.

"Histoire d'un Savant par un Ignorant."—Lady Claud Hamilton is engaged, with the assistance of Professor Tyndall, on a translation of the account of the life and work of M. Pasteur, entitled "Histoire d'un Savant par un Ignorant."

FISH SCALES.—Mr. Charles Collins, nephew of the well-known microscope maker of the same name, has just issued a series of these interesting objects, comprising 48 distinct species.

As *Opaque* slides the scales are splendid objects for the Monocular as well as Binocular, and the skins, though from the same fishes, will be found, when examined, to warrant the repetition.

The scales of the Eel and Perch under the *Polariscope* are pretty well-known, but for variety and beauty the number will bear adding to.

Mr. Davies in his work on the Preparation and Mounting of Microscopic Objects, says, speaking of "Fish Scales," "the variety and beauty of these are quite surprising."

A very interesting article upon "Fish Scales" is to be found in the "Micrographic Dictionary." The following is an extract from the same :—

"Each scale is contained in a distinct sac of the Skin or Cutis, covered externally with its pigment layer and epidermis. The Cutis itself consists of interlacing fibres of areolar tissue with formative cells. The pigment layer is composed of elegant pigment-cells with long processes. Immediately above the upper surface of the Scales lies a very fine membrane, distinct from the Cutis, in which the impressions of the irregularities of surface existing upon the Scales are visible.

"In some fishes, as the Eel, the Scales do not project beyond the surface; hence the Eel is commonly supposed to possess no Scales. They are easily seen, however, in a dried piece of the Skin, mounted in balsam, covered by the Skin with its pigment-cells, the whole forming a very beautiful object."

ATMOSPHERIC DUST.—A very useful little work was published in Paris in 1877 under the authorship of M. Gaston Tissandier, under the title of "*Les Poussières de l'air*," and proved to be an admirable little *brochure* treating upon the subject. The first sixteen pages is devoted to the description of various appliances for catching the dust of the air; and to ensure a quantitative method, great care has been taken to show how the air aspirated has been measured. The apparatus shown on page four fairly resembles the drum, meter and weight of the Alpha Gas Machine, now used for impregnating air with a light hydro-carbon vapour for purposes of illumination, and no doubt this is an excellent form of apparatus for the purpose under discussion.

This study is yet in its infancy, and no doubt much has yet to be discovered ; but it is stated early in the work that the quantities of the sediment or deposit held in suspension by the atmospheric currents is without doubt variable with the speed of the currents, with their hygrometric state, and no doubt changes even with the

seasons, the nature of the soil, &c. Several points are here discussed, which should have the attention of all those who desire to study the subject: the dimensions of the dust; the suspension of the dust in the middle of the air; chemical composition of the atmospheric dust; dust obtained by the evaporation of atmospheric waters; the examination of rain waters; dust and sediment contained in snow water.

Part II. deals with the existence of ferrugineous and magnetic corpuscles in the atmosphere, and no doubt this section will be interesting to our readers, especially those who have had the opportunity of examining Mr. Dancer's slides of the dust from boiler flues and Bessemer converters. Many figures are given by M. Tissandier, which are accurate representations of these slides. Mr. Dancer's paper, already reproduced in abstract in these columns, furnishes an explanation as to how these particles get into the air, and shows also that their source may be purely terrestrial.

The treatise concludes with an account of pollen rains which have at times caused so much consternation amongst the inhabitants in country districts. It is a pity that these subjects are not more studied by those who possess Microscopes in this country.

This little book we recommend to our readers, as affording subject for much thought and study, and we feel sure that any one purchasing it will not be disappointed.

Diamond lenses.—A correspondent writes to ask if we think the diamond will ever come into use again for the construction of the "fronts" of ordinary lenses. Perhaps this query may meet the eye of some practical constructor of objectives, who may be more likely perhaps to solve the problem than we fancy ourselves capable of being.

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Apropos of the subject, Pritchard states in his "Microscopic Cabinet," "a diamond lens exhibits the real object without any sensible aberration, like that produced by single glass lenses."

The Microscopical Bulletin.—Messrs. J. W. Queen & Co., of the States, has sent us a copy of their Journal which has been issued under this name. For the diffusion of Microscopical information it seems to us invaluable, and for the purpose of showing the nature of its contents we have abstracted several of the communications for our present number.

Aylward's Collecting Case.—The maker of this useful companion has now further improved it, so that it may *always* be carried in the pocket without any inconvenience. The new one is rendered more portable than any we have seen before, remaining at the same time an extremely convenient size.

THE NEW SAFETY STAGE.—Mr. Aylward is also making the new safety stage, which was described in a recent number of the Journal of the Royal Microscopical Society. It is an adjunct no Microscopist who exhibits at soirées should be without.

THE USE OF DIAPHRAGMS.—Pritchard says "when day-light is used (which can never be rendered equal to artificial light, by any arrangement, at least for lined objects, with any sort of instrument), vision is much improved by the use of diaphragms; these, however, are inert unless the image of them in the visual pencil is less than that of the aperture of the object glass or magnifier with which they are used.

PRESCRIPTIONS AND RECEIPTS.—M. Vogel points out that in the use of gelatin bromide the addition of an aqueous solution of Chrome-alum to the developer prevents the wrinkling of the film from the action of heat. To 100 c.c. of the developer he adds 2 c.c. of the solution without impairing the sensitiveness. He recommends this to those who make use of gelatin-bromide in warm climates, and also advises them to immerse the plate as soon as developed for two or three minutes in a saturated aqueous solution of common alum.—*J. de Phot. et de Mic.*

Le Bulletin Belge (1880, No. 10) publishes under the name of Dr. Stolze the following prescription for a developer, "which gives to the image an extreme delicacy and softness," and "has the advantage of being under the constant control of the operator."

Sulphate of Iron	5	grammes.
Sulphate of Copper	2	"
Sugar	1	"
Glacial Acetic Acid	1	c.c.
Rainwater	100	"

When all is dissolved the solution is to be filtered.

This developer bears a great resemblance to one of Captain Abney's, which I mentioned in the Annual of the Journal for 1877. It differs from it only in the addition of sugar, and the suppression of alcohol. Apropos of this I would point out that in the prescription as given by me the printer has put 100 c.c. instead of 1,000 c.c.—*J. de Phot. et de Mic.*

Amateurs and artists are strongly prepossessed in favour of Dr. Vogel's emulsion, which, for rapidity, is said to be second only to the gelatin-bromide. The composition is kept secret by its discoverer, but the *Bull. Soc. fr* (1880, No. 11) gives the following receipt:—"14 grammes of common gelatin emulsion, well dried in alcohol, are dissolved in 14 c.c. of glacial acetic acid, at a temperature of from 30° c. to 35° c. Then are added, in small quantities, and with constant stirring, 28 c.c. of a mixture of $\frac{1}{4}$

glacial acetic acid, and $\frac{3}{4}$ collodion in 40 parts. The whole is then laid on by the help of 8 c.c. or 20 c.c. of a mixture of one part acetic acid, and three parts absolute alcohol. To prepare plates with this emulsion they must first be covered with a solution of caoutchouc. The development is made with pyrogallic acid. The Vogel Emulsion may be obtained from M. Schœffner, 10-12, Passage du Buisson-Saint-Louis.—*J. de Phot. et de Mic.*

"THE NATURALISTS' WORLD" is a monthly magazine which commenced its existence in January of the present year. Its cost is twopence, and its aim seems to be that of popularising the study of scientific subjects. We wish the magazine every success, but fear the first number has not proved sufficiently attractive, nor does it set forth in the most satisfactory manner the idea of the intended practical nature of the work. The Publishers are Swan Sonnenschein & Co., of London.

PROCEEDINGS OF THE AMERICAN SOCIETY OF MICROSCOPISTS. —We have received this very interesting *brochure* of 270 pages, and offer our best thanks to the donors of the same. We congratulate the members of this Society upon the very excellent manner in which the sixth Annual Meeting was managed at Chicago, and we think that the details of this meeting as published in pages 248-268 may be studied with profit by every member of our Microscopical Societies at home. We shall endeavour during the year to reproduce some of the papers read at the Chicago meeting.

PROF. ABBE'S PAPERS.—We have been asked by several of our subscribers to reproduce *all* these papers which have from time to time appeared on the microscope. Believing this to be one method of propagating a true knowledge of microscopy, we shall endeavour to devote as much space to the purpose as we can well afford.

"EVENINGS WITH THE MICROSCOPE."—Next month we hope to be able to commence a series of papers under the above heading, especially intended for beginners, and written in a popular style; perhaps the Honorary Secretaries of Microscopical Societies will call the attention of their junior members to this.

LEITZ OIL IMMERSION OBJECTIVES.—Owing to want of space this month, we are compelled to postpone an account of these lenses to our next issue. Some remarks upon oil immersion lenses generally may be found in the article "Infusoria from a water-butt" in the present number.

EARLY OBJECTIVES.—We have been promised the loan of several of the earlier objectives, which were used over the markings of diatoms, on their first discovery. The comparison of these glasses with those of the present day will be interesting.

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

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THE FORMS, ORIGIN, AND DEVELOPMENT OF THE TEETH.

By Parsons Shaw, D.D.S., (U.S.A.)

(Continued from page 89.)

ALTHOUGH we have seen that the beginning of dental structures is in the scales of fishes, we have not now the time to trace the various modifications of the substances of which they are composed up to the teeth of the higher animals. Allowing for all the transitional forms, a tooth is generally composed of three hard parts, namely—the enamel, the dentine, and the cementum, and a soft structure called the pulp. Nowhere are these seen to more perfection than in the human teeth.

The enamel is the hardest part of the animal body; it varies in different animals; it never has more than five per cent., and often scarcely a trace of organic matter, and it is chiefly made up of lime salts. It is often said to be structureless, but that I cannot admit, for the microscopist often discovers that patient investigation with better powers reveals structure where it was too readily assumed not to exist. If the enamel is treated with dilute hydrochloric acid, it is dissolved away in the axial parts, so that if the section be transverse a fenestrated mass remains. In carnivorous and omnivorous animals the enamel covers all that part of the tooth exposed in the mouth; and it also covers the incisors, and flows, as we have seen, through the body of the molars of the herbivora. The dentine constitutes in the human tooth the central and principal part of the body to such an extent that if only this structure were left it would show the distinct form of the tooth. The dentine is everywhere penetrated by what are known as the dentinal tubes. Each tube starts by an open circular mouth upon the surface of the pulp cavity, from whence it runs outwards towards the periphery of the dentine, which it does not usually reach, but breaks up into

branches at a little distance beneath the surface; consequently, in longitudinal sections the tubes are seen running from the pulp cavity toward the periphery. But in a transverse section across the neck of the tooth there may be seen the ends of the tubes where they present the appearance of the ends of pipe stems fused together. The number of tubes in each tooth is so great that no one has to my knowledge undertaken to estimate them. Near the pulp they are closely packed together, but near the outside of the tooth they are more widely separated. The dentinal tubes do not pursue a straight line, but describe curves, some of the larger of which have been compared to the italic letter *f*. There can be no doubt that in many cases the course of the tubes is that of an elongated spiral; and I fancy this is really the course they always take. Not only do the tubes divide as they pass outwards, but their branches anastomose with the branches of other tubes, thereby forming loops. In the crown of the human tooth there are but few branches except near the enamel, but in the root they are more numerous. Sometimes the tubes pass beyond the dentine, and anastomose with the canaliculi of the cementum. In marsupials and some other animals the enamel is constantly penetrated by the tubes, and not unfrequently they are seen in the human enamel. Mr. Tomes is surprised to find the human enamel penetrated by the tubes, but this I take to be a "harking back" to a former type. There is what appears to be an excellent illustration of this reversion in the last number of the "Cosmos," an American dental journal. Dr. Knowlton, of Belden, Ohio, records a case of abnormal dentition, where a tooth with five cusps came in the place of a central incisor. If you will examine the bicuspid teeth in a human jaw you will see they are made up of a combination of cones, three in front and three behind. Then look at the canine teeth, and you will see they are modifications of the bicuspids by a depression of the inside cusps. Then examine the incisors, and you will see the same outlines of the three cones as on the outside of the bicuspids, showing they are modifications of teeth that were at one time not wedge-shaped but multi-cuspid. Now, unite the middle cone of this abnormal tooth with the two cones on each side of it, and depress the other two and you have an incisor. Go back still further, and you find among fishes the aplodactylus has teeth made up of three-cusp combinations, each of which is very similar to this abnormal tooth except in the number of its cusps. In the primates, carnivora, &c., the cementum is confined to the roots of the teeth: but it runs through the bodies of the teeth of the herbivora, as I have shown. It is thought the cementum has a *tendency* to form also over the crowns of the human teeth as well as in the animals named, and I think this hypothesis is correct, for it is sometimes found in the depressions

of human molars. The probability is that the cementum did spread over the ancestral forms of the human teeth, but that it has been so persistently worn away for so long a time that it has lost the tendency to cover this part of the tooth. There is some conformation of this supposition in the teeth of the wappiti deer. You see that the cementum does form over the side of the tooth, as in the horse; and I suppose it did extend all over the crown just as in this specimen of the tooth of a young horse just cut through the gum. But in the deer the cementum is now nearly gone, although enough remains to show the tendency to form. Although I have said that teeth are not bones, the cementum is osseous in character, being formed by membranous ossification upon the roots of human and some other teeth, and by ossification in a fibrous cartilage, according to Magitot, in the teeth of horses, &c. The vascular and nerve supply in the cementum is derived partly from the pulp, but in the greatest degree from the alveolar dental periosteum. The pulp of the tooth is really the slightly metamorphosed dental germ now enveloped by the hard structures of the tooth. The pulp is made of a mucoid, gelatinous matrix, containing abundant cells, which are most numerous near the periphery. The pulp is firmer and denser on its surface than in its body, which gives it the appearance of being covered by a definite membrane. The vessels of the pulp are very numerous; indeed, it is almost wholly composed of vessels. Three or more arteries enter the apical foramen and spread out into branches, which finally form a capillary plexus. No lymphatics are known to occur in the pulp. There enter by the same apical foramen one large and three or four smaller nerves, which, after pursuing a parallel course and giving off branches which anastomose but little, eventually form into a rich plexus. The nerve supply in the dentine is wholly derived through the pulp, and that of the enamel as well, if any is found in this substance.

How the teeth arise and are formed is not yet quite made out. About the year 1837, Goodsir told us, in so precise a manner that it seemed there could be but little more to learn, exactly how the germs of the teeth arose, and how they afterwards developed. Goodsir's opinions got into all our text books, where they remain with few exceptions to this day, and are quoted and taught as if they had not been contradicted by later and more accurate investigation and shown to be quite untenable. Instead of the matter being so easily settled, the most patient investigation by some of the best minds in Europe and America has not yet determined many of the most important questions connected with the subject. What we know for something like certainty I will state in as concise a form as possible.

In the epithelium there arises what is termed the enamel organ.

This first assumes a spherical form, but it soon changes in the course of development into a cap-like investment to the dentine bulb, and is thenceforth modelled by the latter into whatever shape it assumes, being thickest over the apex of the bulb and thinning down as it approaches the base. In all probability, the enamel is formed by the actual conversion of the enamel cells. Almost simultaneously there arises as an opaque point in the submucous tissue, but in close proximity to the enamel organ, the dentine bulb. This soon begins to assume the form of the apex of the future tooth, becoming simply conical if a canine, but if it is to be a tooth with more than one cusp, sending as many conelike depressions into the enamel organ, and occupying these depressions, as there are points in the new tooth. Coincidently with these changes in the dentine bulb, the layer of cells forming its surface, which is now in close relation with the enamel cells, becomes differentiated from the parts beneath it and forms the odontoblasts, or the cells which become dentine by calcification. This process begins at the points of each of these cones, and progresses until they coalesce, as may be seen in this partly-formed monkey's tooth. Before calcification can take place, the dentine bulb is metamorphosed into the dentinal matrix, or tooth cartilage. This organ assumes just the shape and size of the dentine, and can easily be obtained in the formed tooth by dissolving out the salts of the dentine by means of hydrochloric acid, which will leave the matrix complete. The walls of the dentinal tubes, called the dentinal sheaths, are formed in and are a part of this matrix; and if the process of its decalcification be carried far enough, it can be so much destroyed by the acid that the sheaths alone will remain as a transparent slime. This demonstrates not only that the dentinal tubes have definite walls, but that they are singularly indestructible. Indeed, they are so persistent as to be found in fossil teeth. It is around the sheaths of these walls that the calcification of the matrix takes place, and thereby are formed the tubes seen in the sections I have prepared for the microscope. Simultaneously with the formation of the dentinal matrix, its calcification commences; and also simultaneously with the general metamorphosis of the dentine bulb, is formed the dentinal fibrils. These fibrils are an extension of the pulp into the tubes, and are little else than the unaltered protoplasm of the odontoblast cells,—that is, the tooth-germ cells. Thus we have three stages in the conversion of the bulb going on at once. First, there are the dentinal fibrils. Second, there is forming the dentinal sheath to enclose the fibrils and as walls to the tubes, a substance which lies just on the borderland of calcification, if it really does not calcify to some extent, which I think it does. And third, there is the calcifying matrix, which fills all the space between the sheaths. The dentine begins

to be formed upon the surface of the dentine bulb, and the progress of calcification is from without inward, from the periphery toward the pulp cavity; so that no portion of the dentine can receive any increase externally, but all the additions must be upon the interior of the calcified cap. This process goes on until the tooth-germ surrounds itself with the dentine, when it is now no longer known as the "dentine bulb," or "papilla," or "tooth germ," but as the "tooth pulp" which I have described. But, notwithstanding the altered environment of the pulp, it never quite loses its formative powers, as may be seen by some illustrations by Magitot, a patient French investigator, who shows that the near approach of decay may irritate the odontoblasts into renewed action, whereby new dentine is put forward over the threatened part. As the dentinal fibrils are but prolongations of the protoplasm of the odontoblast or formative cells of the tooth germ, it is not difficult to see that when these are reached by the decay penetrating the dentine these cells are set into renewed action. It is not to be supposed, however, that no alteration takes place in the dentine bulb after the bulk of its work is done, for investigation shows that the odontoblast cells somewhat change their outline when the day of their activity is over. When the crown of the tooth is finished, the neck and root, or roots, begin to form. As each portion of the dentine of the root is completed, it is coated over with a closely adherent vascular membrane, which presents on the inner or dentinal surface a layer of large osteoblast cells. It is by the calcification of these cells that the cementum is formed in those teeth where it only covers the roots. When the cementum has formed, this membrane remains to be now known as the alveolar dental periosteum before mentioned. Kölliker thinks, what is highly probable, that the cementum is formed in isolated scales, which eventually coalesce with one another; and Magitot thinks the formation of the cement in the herbivora is by the ossification of cartilage, and not by membraneous action.

The dentinal fibrils are most interesting as well as important structures. Everyone who has had a tooth filled has been feelingly persuaded of their existence. In removing the decalcified dentine prior to filling up the cavity formed by the decay, the instrument cuts across numberless fibrils, each one of which is a centre of sensation. All the common talk about the "nerve of the tooth being exposed," as if there was one distinct nerve to each tooth, and it was the laying of that bare which caused the continuous pain we know as tooth-ache, will be seen to be a mistake from what I have said of the pulp; and the use of such a phrase as "the nerve of the tooth" is unscientific and misleading. In excavating a cavity in the tooth, while the pain may be keen for the time, it will only be continuous until the decay has actually reached the

pulp, and will stop almost immediately after the instrument is removed. Moreover, in preparing a tooth for filling the sensation in the fibrils will be found greatest at their ends and away from the pulp, just as we should expect in all sensation conductors. We know next to nothing of the fibrils from actual observation; but as they are simply prolongations of the formative cells of the pulp, we may safely conclude they play the most important part in the calcification by which the tubes and sheaths become less as age advances. The practical experience of the observant dentist also gives further evidence of the character of the fibrils which other investigation is unable to make out. When a tooth has been filled in the most thorough manner, it will be found that it is subject for a time to the influence of heat and cold; and the more thoroughly the cavity is prepared, and the firmer the filling is inserted, the more is this apt to be the case. After a time this will cease. There can be but one explanation to these phenomena. The removal of the decayed part of the tooth exposes the ends of the tubes and the contained fibrils. The gold that fills up the cavity is an excellent conductor of heat and cold. Therefore, so long as the ends of the tubes remain open the fibrils are exposed, and the effect of heat and cold remains; but when the fibrils, acting as odontoblasts, have thrown out new dentine to fill up the exposed ends of the dentinal tubes, the sensation ceases. Although it is not common to speak of the fibrils as nerves,—and in the usual sense of that term I do not contend they are nerves,—there can be no doubt they do convey sensation to all parts of the dentine; and this demonstrates to my mind that primitive nerve organisation may be looked for where it has not heretofore been suspected, because the nerve sheaths have not been made out.

I have thus, in this very imperfect manner, endeavoured to sketch the history, uses, and organisation of the teeth, so as to give something like a guide to the student of these organs. In so doing I have followed the theory of evolution, as put forth by Dr. Darwen; for the simple reason that in no other way I could form opinion on this matter. Many may object to some of my conclusions, and even the proved facts, for the reason that they involve this doctrine. In reply, I can only say that where there is an ardent wish to know the truth, a real reverence for things sacred, and a profound love for the Divine Creator, as the laws of life are studied, and the conclusion accepted that they are revealed through the order which evolution indicates, so far from a check being given to holy aspirations the mind will have reached a new standpoint from which to contemplate the wonderful mysteries of His never-ceasing wisdom and goodness.

THE PROPOSED SEA BIOLOGICAL ESTABLISHMENT.

THE *Morning Post* remarks that a meeting in the rooms of the Royal Society, under the presidency of Professor Huxley, and attended by many of our most eminent scientists and naturalists, indicates at least one highly important result of the Fisheries Exhibition, in having directed attention to the natural deficiencies in knowledge and powers of research concerning the organisation and habits of the denizens of the deep. To few countries could biological study in this direction produce greater benefits, as by none, perhaps, could that study be more easily and effectually pursued, and yet it has, we think, met with strange disregard in our sea-girt kingdom. There can be no doubt that the scheme will meet with warm and universal approval. We are essentially a sea-loving people, and, once the possibility is placed before us, shall be eager to learn the solution of some of the many mysteries connected with its inhabitants. To the naturalist these discoveries will be precious for the sake of the advancement of biological science; to the thousands of fishermen on our coasts they will doubtless bring less toil and richer harvests; and in the interests of the general public they will serve to increase the fish supply whilst preserving certain species from the extinction with which they are now threatened.

EXTRACTS FROM MR. H. E. FRIPP'S TRANSLATION OF PROFESSOR ABBE'S PAPER ON THE MICROSCOPE.

Monthly Microscopical Journal, vol. xiv., page 191.

(Continued from page 96.)

THEORETICAL study of the aberrations of the image-forming rays, and practical experience involving the application of methods to be hereinafter described, and the careful testing of a considerable number of objectives of recent date from the best workshops on both sides of the Channel, have led Professor Abbe to the conclusion that the numerical value of "necessary amplification" yet arrived at or attainable at present, is altogether much lower than might be supposed from the liberal way in which microscopists deal with thousands and tens of thousands. According to his

experience, the capacity of the most perfect objectives, the usual forms of illumination being assumed, is exhausted with an eight-fold *angular* amplification, so that every detail that can be possibly delineated by an objective in its "virtual" image is certainly accessible to any eye possessing normal vision, when the tube and ocular, taken together, represent a telescopic magnifying power of eight times. Even this performance is only reached in the case of low and middle power objectives; for when the focal length is less than $\frac{1}{8}$ inch, the relative perfection of construction perceptibly fails, on account of the rapidly accumulating technical difficulties, and there certainly does not exist an objective of $\frac{1}{25}$ inch focus whose optical capacity exceeds a fivefold *angular* amplification.

From all this may be gathered how utterly futile any efforts to obtain disproportionately high amplifications by means of specially constructed eye-pieces must prove; and, as regards any expectation of exalting the performance of the instrument by further shortening of the focal length of the objective, there stands in the way one objection, which, in the present state of our knowledge, is absolute and insuperable—namely, that the imperfections resulting from residual aberrations and defective technical manipulation increase with every addition of magnifying power. This form of diffraction, likewise, turns the image of each point in an object into a dispersive circle of greater or less diameter; but the resulting diminution of optical capacity, while scarcely noticeable in objectives of moderate power, compared with the effect of residual aberrations, becomes very serious with the higher powers. Assuming the magnitude of angle of aperture 180° in air, which cannot be exceeded beyond a few degrees, even by immersion systems, we find, e. g. for an amplification of 1000, the diameter $= \frac{1}{50}$ inch, and for amplification of $5000 = \frac{1}{250}$ inch, without reference to the mode in which the amplification is obtained (through objective and ocular). And if we would know what conditions are involved in such amplifications—as, for instance, 5000 fold—we have only to make a puncture of $\frac{1}{250}$ inch diameter with a needle in a card or piece of tinfoil, and through this opening to look at some brightly illuminated object, which has well-defined edges (e. g. a candle flame), and we shall have before our eye of what must be the appearance of the outlines of a microscopic object magnified 5000 times, even if the microscope itself were absolutely perfect, the diffractive effect excepted.*

Taking all these circumstances into consideration, it must be concluded that no material exaltation of the absolute power of the

* Due to smallness of aperture of a minute lens, and to be carefully distinguished from the diffraction which is caused by the *structure* of objects.

microscope, beyond what is attainable at present with objectives of $\frac{1}{25}$ inch focal length, is to be expected in the future, either by shortening of focus or by further improvement of construction. And as there exists at this moment no microscope whose *serviceable* magnifying power reaches even to 4000, so will there be none in the future. On the contrary, the facts just stated show that amplifications of less than half 4000—such as are readily obtained with objectives of $\frac{1}{25}$ inch, and seem really *serviceable*—are nevertheless not available in practice. The final inference from these data is that improvement of the microscope should no longer be sought for by aiming at still higher magnifying power and amplification, but rather at a more correct performance of the middle and moderately high powers. *It will be a real advance of the optician's art, and of infinite service to the scientific use of the microscope, when we succeed in accomplishing with objectives of $\frac{1}{6}$th and $\frac{1}{8}$th what is now only attained with much higher powers. Such an aim is within the range of what is possible.*

In the account of Professor Abbe's researches, to be hereafter published, new and exact methods will be given by which every determinable point in the construction of the microscope, e. g. focal length of each lens, angle of aperture, character and limits of objective and ocular functions may be empirically ascertained; and, in addition to this, a mode of procedure described which renders it possible, with very simple means, to examine in instruments already made, every fault of definition of image, and thus to determine their relative excellence. The methods commonly recommended for testing the state of spherical and chromatic correction of the objective are not adequate to the actual requirements of the case, and quite fail to explain the true character of the aberrations.

The principle upon which the mode of proceeding to which reference has been made above, may be here generally indicated. As test-object, a preparation is used which presents only sharply outlined black and white lines alternating with each other, and *lying in the same plane*, so that no deviation can occur in the course of the rays transmitted through it. A preparation of this kind, sufficiently perfect for all practical purposes, may be made by ruling groups of lines, with the aid of a dividing machine, on the metallic film of silver or gold fixed by known methods on glass, and having no greater thickness than a fractional part of a micro-millimeter (1 micro-mm. = $\frac{1}{25000}$ inch). Covering glasses of various thicknesses (accurately measured) are ruled on their under surfaces with lines $\frac{1}{250}$ to $\frac{1}{1250}$ to the inch, and cemented on a glass slide with balsam, one beside the other. A preparation of this kind serves for the highest as well as lowest powers. The illumination must be such that light may be reflected simultaneously from several sides upon the object, and means provided for regulating at will the

course of any pencil entering within the angle of aperture of the objective to be tested.

The testing process has for its aim to view the co-operation of every zone of the aperture, whether central or peripheral, and yet, at the same time, to be able to distinguish and recognise the images which each zone delivers separately. For this purpose the illumination is so regulated that every zone of the aperture shall be represented in the image formed at the upper focal plane by tracks of the entering pencils of light, yet so that for each zone a small streak only of light be let in, and that the tracks be kept as widely apart from each other as possible. If an objective be absolutely perfect, all these images should blend *with one setting of focus* into a single, clear, colourless picture.

A test image of this kind at once lays bare in all particulars the whole state of correction of the microscope. With the aid which theory offers to the diagnosis of the various aberrations, a comparison of the coloured borders of the separate partial images, and an examination of their lateral separation and their differences of level, as well in the middle as in the peripheral zones of the entire field, suffice for an accurate definition of the nature and amount of the several errors of correction, each of them appearing in its own primary form.

Assuming the theoretical knowledge and practical experience necessary to carry out such an inquiry properly, and to estimate its results correctly, the mode of procedure above described affords so exhaustive an analysis of the qualities of an objective, that when, in addition, its focal length and angle of aperture are ascertained, its whole capacity of performance may be determined beforehand. *Whoever has once examined in this manner even good objectives which have proved to be excellent in practice, will be as little disposed to accept childish assertions of their perfectness as to advance on his part absurd pretensions which no one has yet made good.*

That the performance of the microscope does not always depend solely on the geometrical perfection of the image, but also, in addition to this, in certain classes of objects, upon amount of angular aperture, is a fact long recognized. The exact significance of this fact has nevertheless remained as problematical as the exact nature of the quality of "resolving" or discriminating power. It remained a question, What value might be assigned to the quality thus related with angular aperture, and does its significance extend any farther than to certain cases in which shade effects were supposed to be produced by oblique illumination?

In the endeavour to establish a theoretical basis for the construction of the microscope, it was a matter of the first importance to define the exact function of angular aperture in the normal performance of the microscope, lest I should fall into

a misdirection of my labours towards aims of very problematical worth.

As, then, it was important above all things to ascertain more exactly than has been hitherto set forth the actual facts respecting the operation and effect of angular aperture, I endeavoured to determine by experiment in what cases a distinct advantage resulted from larger angular aperture, and in what cases no such advantage could be perceived. For this purpose a series of objectives, differing widely in focal length and angular aperture, were constructed, according to my calculations, and their accuracy tested, so as to afford a certainty of correctness. The test-objects employed included prepared insect scales of various kinds, diatom valves, striped muscle fibre, diamond-ruled lines on glass, groups of lines on silvered glass, fine and coarse powdered substances, and, besides these, the minute optical images of natural objects (lattice bars, wire-net) obtained by means of air-bubbles, or, preferably, by objectives of short focus, fitted to the stage of the microscope.

These experiments yielded the following results:

(i.) So long as the angle of aperture remains within such limits that no noticeable diminution of sharpness of image results from its diffraction effect, no sensible improvement in the delineation of the outlines of the object takes place, provided these parts are not of less size than $\frac{1}{2500}$ inch.

(ii.) On the other hand, the difference is wholly in favour of the larger aperture for every object which yields details minuter than the limits above given; and this quite irrespective of the question whether such details are due to unevenness of surface or to unequal transparency in an infinitely thin layer, or whether the detail takes the form of striation, granulation, trelliswork, or images of natural objects reflected from bubbles or produced by refraction of lenses.

(iii.) The smaller the linear dimension of such details, so much the larger must be the angle of aperture of the objective, if they are to be made out with any definite kind of illumination, e. g. whether purely central or very oblique: and this irrespective of the more or less marked character of the delineation and of the focal length and necessary amplifying power of the objective.

(iv.) When the detail in the real object appears in the form of striation, groups of lines, &c., a given angular aperture always reaches much finer details with oblique than direct illuminations, and this irrespective of the circumstance that the constitution of the object admits or excludes the possibility of shade effects.

(v.) A structure of the supposed kind, which is not revealed by an objective used with direct illumination, will not be rendered visible by inclining the *object itself* at any angle to the axis of the microscope, even when lying at right angles with the axis, it is

perfectly resolved by oblique illuminations. Resolution, however, follows at once when the incident light is directed perpendicularly to the plane of the object, as it lies inclined to the axis. Hence the increased effect of oblique illumination depends solely on the inclination of the rays towards the axis of the instrument, and not upon the oblique incidence of light on the object.*

The facts here brought forward show, on the one hand, the reality of a special optical quality, directly related with the *angular aperture* of the objective, yet independent of any special perfection or amplifying power possessed by it, and also show it to be a "resolving" power or capacity of separating minute detail, conformably with the literal sense of the term employed. On the other hand, they show unequivocally that the delineation of images of minute details of structure must take place under conditions essentially different from those under which the contour outlines of larger parts are formed. In all cases where a "resolving" power of this kind is in operation, the reunion of rays proceeding from the several points of the object in the focal plane of the image is most certainly not to be accounted an adequate explanation of the images of such details of the object, for on such a supposition the differences would remain absolutely inexplicable. The result, then, of this preliminary study is to give the following form to the inquiry, namely, to find out the special causes *outside* the microscope which operate in the formation of images of small structural details, and then to determine individually the mode and manner of their intervention.

(To be continued.)

PEBRINE.

THE suggestion that the shortcomings of last year's China silk crop have been due to an epidemic of *pebrine*, the disease which nearly extinguished the French and Italian silk-growing industries, appears to have attracted a good deal of attention not only in China but in Europe. According to the *North China Herald* applications have been made in Shanghai for specimens of China silkworms' eggs. These applications have been attended to, and consignments are now under process of investigation and culture in Europe by experts. Some time must, of course, elapse

* Vide Wenham, in 'Monthly Microscopical Journal,' April 1, "On a Method of obtaining Oblique Vision of Surface of Structure," &c. The optical principle enunciated by Mr. Wenham is totally irreconcilable with Professor Abbe's theory and experimental investigations.

before a definite answer can be returned. One of the most important facts disclosed by Pasteur was, that the germ may exist undetected in the egg. Now, while a worm developed from an infected egg will invariably fail to spin a cocoon, worms subsequently infected by contagion may, nevertheless, spin very valuable cocoons. But if such infected worms be preserved for seed purposes the moths will certainly produce infected eggs, and be the parents of a diseased race. It is, therefore, in the later stages of the life-history of the insect that the noxious corpuscles must be sought for; and Pasteur's method, which has revived the silk-rearing industries of France and Italy, is based on a microscopic examination of the chrysalis and moth with a view to ensuring the health of the succeeding generation by the selection of the eggs of those moths only which are themselves healthy. There would be nothing surprising in the development of an epidemic in China. The malady is not a new one; there is historical evidence that it has always committed ravages of greater or less extent in Asia as well as in Europe. The efforts to produce a large crop are themselves favourable to its development, as they result in more crowded cultures and diminished care in attending to the sanitary conditions under which the worms should be reared. Finally, failure in any locality tends to develop an epidemic, because it leads to an increased trade in eggs. With the increase of demand the cultivators and dealers become careless as to the quality of the eggs, and thus diseased cultures may be spread far and wide, the intensity of the affection and the probability of contagion being increased continually. This was the result throughout Europe, and to some extent in Asia, when the development of the epidemic in France and Italy led to a demand for eggs wherever sericiculture was carried on. Indeed, M. Pasteur predicted an outbreak of *pebrine* even in Japan itself, almost the only quarter from which a few years ago healthy eggs could be obtained. In China it is admitted that the disease has always prevailed more or less, and the natives have long adopted a method of subjecting the eggs to baths of salt and water, and lime and water, which, it is held, prevent the sickly eggs from hatching. This, however, is not a very scientific method, and there is good reason to doubt whether it is really *pebrine* or the other ill to which silkworms are liable—*flacherie*—against which such baths are effective. The thing is to secure a large supply of healthy eggs, not merely to kill off those intended for hatching—obviously a costly process for the cultivator. Should the Chinese be in real danger of losing their silk-rearing industry from *pebrine*, and be induced to avail themselves of M. Pasteur's method for its restoration, they will be taught that Western science is worth something after all. — *Manchester Guardian.*

THE CHOLERA GERM.

THE discovery of the cholera bacillus by Koch and his colleagues, Fischer and Gaffky, and the announcement of its presence in tank water, appear to have caused even a greater sensation in India than in Europe. The Indian papers teem with articles and letters on the subject. Whether Prince Bismarck's popularity is waning or not in his own country, he is just now being regarded in India as one of the foremost philanthropists of the age. The conduct of England, which "makes laws for the prevention of scientific discovery," is contrasted with that of Germany, which pays the cost of sending a Commission to our great dependency in order to discover for us the nature of the disease most dreaded there.

It was a happy thought of Prince Bismarck—who has as much on his hands as Mr. Gladstone has—to send a Commission of Inquiry to Egypt and India on this very difficult and important errand, and some people may be inclined to wonder why it was left to a Continental Government to do such a thing: why Englishmen have not had the entire credit—which they well might have had—of making this very interesting discovery. As it is, although the actual proof of identification of the microscopic organism which must in future be recognised as the cholera germ as the cause of the disease is exhibited by an English medical gentleman in India—Dr. Vincent Richards—a very large share of the credit for the discovery unquestionably belongs to Dr. Koch. Dr. Koch's researches in Egypt, though inconclusive, encouraged him to believe that cholera had a parasitic origin, and he did discover, in the cholera patients he examined, a certain kind of bacilli, "microscopic creatures in shape like a small ruler or piece of stick." He did not, however, discover the source of these outside the human system. Hence it might have been hastily assumed that the parasites were a result, and not a cause, of the disease. Dr. Koch and his companions, however, proceeded to Calcutta, and while they were there, an epidemic of cholera broke out in a native quarter of the city where there was a pond, the water of which was used for drinking and bathing purposes. This water was microscopically examined by Dr. Koch, who discovered in it—what he had not previously succeeded in discovering apart from the human subject—the same kind of parasites which he found in the cholera patients, and which are quite distinct from those that are known to produce consumption, scarlet fever, and foot-and-mouth disease. Moreover, coincident with the subsidence of the outbreak in Calcutta, it was found that the parasites became scarcer in the water. Unfortunately for Dr. Koch, all his attempts by way of experiment to reproduce the disease by inoculating

various dumb animals—mingling the parasitic water with their food—proved failures. The hot season arrived, and it became necessary for Dr. Koch and his colleagues to return to Germany, without establishing the last link in the chain of evidence needful for the establishment of a theory the soundness of which the German scientists were convinced of. Happily the missing link has not much longer remained undiscovered. Dr. Vincent Richards, civil surgeon of Goalunds, has accomplished what Dr. Koch failed to do. According to a telegram received in England, the *Calcutta Englishman* announces that Dr. Richards has succeeded in communicating what is believed to have been genuine cholera, by means of the cholera bacilli, to a pig, which died in three hours after the infectious matter had been administered. We have yet, of course, to learn how it happens that this experiment with a pig has so wonderfully succeeded while many previous experiments failed. But once successful, the experiment will no doubt be repeated until ample confirmation is obtained. In the meantime it will perhaps be comforting to English people to know that no "germs" of this kind are known to exist in connection with such diseases as dysentery and diarrhœa, which have a somewhat strong resemblance to cholera.

Koch's extremely valuable discoveries at least indicate methods which would make it absolutely impossible for any known case to become the origin of an epidemic. As regards the probable mode of diffusion and with special reference to the discovery of the bacillus in tank water, it is significant that a cholera outbreak in Bombay which cost upwards of a hundred lives coincided with a three days' stoppage of the Vehar water-supply through the bursting of a pipe, many of the people being compelled to have recourse to the wells in consequence.

THE APPLICATION OF QUANTITATIVE METHODS TO THE STUDY OF CERTAIN BIOLOGICAL QUESTIONS.

A Lecture delivered by H. C. Sorby, Esq., LL.D., F.R.S., at the Annual Soirée of the Manchester Microscopical Society, February, 1884.

DR. SORBY said the subject of his Lecture was "On the Application of Quantitative Methods in the Study of certain Biological Questions." He thought it was pretty well admitted on all hands that the present condition of practical science was due to a large extent to the application of quantitative methods. These methods

had been applied to physics and chemistry, but hitherto almost insuperable difficulties had prevented their application in the case of biological questions. He proposed to show that it was possible to deal with certain matters in this manner. Possibly his training had to some extent led him to carry out these investigations. About two years ago he was requested to undertake a long series of investigations with respect to the matters held in suspension in the water of the river Thames in order to give evidence before a Royal Commission appointed to report upon the question, and in order to do the work effectually, he lived for three months on the Thames in his own yacht. As the result of his various researches, he had come to the conclusion that the removal of impure matter discharged into our rivers by sewers was by no means a chemical question, as was generally supposed, but to a large extent a biological one. He found that these impurities formed the food for some of the minute living organisms which swam in the water. He then considered it most important to decide the number of these minute creatures per gallon of water, and devised a method which he believed had not been tried before. The course he adopted was to pass nine gallons of river water through a very fine wire gauze sieve of about 120 meshes to the inch, and all the minute creatures upwards of $\frac{1}{200}$ of an inch in diameter remained. The sieve was then washed out with clean water; the objects counted in a glass trough which was repeatedly filled. The counting was facilitated by the different ways in which the objects moved about in the water, which enabled him the more readily to distinguish them. In addition to the small adult animals there were the larvæ of larger ones. He was surprised to find such a large number of animals he did not expect to meet with. He took with him various poisons for the purpose of killing the objects, but he found a more simple method was to add a little alcohol, and after pouring off the salt water, to place them in rain water to which an equal measure of alcohol had been added, which prevents the precipitation of sulphate of lime. After pouring this off, the specimens were kept in alcohol.

The following tables were thrown on the screen by means of the oxyhydrogen lantern :—

Fresh Water Researches.

Number per Nine Gallons of Water.

	Pure River.	Thames.	Impure River.
Cyclops...	4.1	26.3	1.5
Daphnia..	2.0	.9	1.2
Cypris.....	1.1	0	0
Annelida..	1.4	.4	10.0

Table showing the number of objects per nine gallons of water taken from different localities, chiefly near Maldon, Essex :—

	River.	Canal.	Ditch.	Canal Basin.	Erith
Cyclops	6.9	25.4	51.0	39	26.4
Daphnia	.4	3.0	2.0	177	.9
Cypris	2.0	10.8	36.0	6.3	.0
Rotifers	.0	.4	.0	.7	.0
Annelids, &c.	.6	1.6	4.0	.3	.4
After September 2,—					Pond.
Cyclops	2.0	3.0	26.2	6.6	866.
Daphnia	.9	1.3	.5	4.8	277.
Cypris	1.0	2.0	6.0	0.7	76.
Rotifers	.6	.3	1.0	38.6	27.
Annelids, &c.	.2	.0	1.0	.5	42.

The large number of Cyclops in the Thames, as shown in the first table, is probably due to the food supplied in the sewage. He had kept Cyclops on sewage matter since last May. If kept in clear water they died in a week. The average number of the excrements of these Entomostraca held in suspension at half-ebb tide at Erith is 170,000 per gallon. By calculating the different per centages of the several objects, they would perceive at a glance the change produced by altered conditions. Taking the Thames as an example, they would find that although there was a considerable reduction, there was no change in relationship until we came to the salter water at Gravesend. He was in hopes that by the study of this question it might be possible to utilize this method as a means of testing how far certain waters were or were not fit for drinking purposes, and also to form some judgment with regard to impurities discharged into rivers. He found that Cyclops was killed when it reached salt water, and he thought it would be interesting if they could form some estimate of the number destroyed in this manner. Engineers had kindly supplied him with data, and he had calculated the quantity of organisms destroyed per day by two different methods, and it was remarkable how nearly they agreed. Without going fully into particulars, the conclusion he came to was that at the end of May or the beginning of June the number of these Entomostraca killed by salt water was about 2600 millions per day.

Having exhibited on the screen drawings of the animals found, and described their general character, he gave a few details as to the numbers found per nine gallons of water in various localities :—

	Swail.	Swin.	Pont.	Colne.	Screws.	Stones.
Ascidians	44	4	45	5	4	$\frac{1}{2}$
Annelids	38	1	9	11	6	0
Rotifers	43	18	20	52	84	0
Gammerus	$\frac{1}{8}$	0	0	$\frac{1}{72}$	$\frac{1}{4}$	$\frac{1}{2}$

	Swail.	Swin.	Pont.	Colne.	Screws.	Stones.
Copepoda	60	16	212	103	274	37
Balanus	36	11	36	54	13	8
Noctiluca	8485	384	90	62	63	0
Totals	8706	434	412	287	444	46

This table shows a striking difference in numbers within so small a distance as 50 miles. He did not wish to press his conclusions in any way, since the relative numbers might be very different another year. One point which struck him was the extraordinary number of objects in some localities. He thought further investigation would prove that they are the food of larger animals, and throw light on the distribution of certain fishes, and on the question of oyster culture. At one place, where they occur in very small number, £60,000 had been spent in oyster culture with no success, which seemed to show that there was not the food for them which was found elsewhere, where oysters flourish remarkably well. There was a vast amount of work to be done in the way of investigation. The following table shows that some of the animals occur in greatest numbers near the surface, others lower down, and some at the bottom. It also shows how some vary according to the state of the tide.

	Surface.	4 feet.	Bottom.	High Water.	Ebb Flow.	Low Water.
Ascidians	9	2	4	6	$4\frac{1}{2}$	3
Annelids	12	14	25	7	34	14
Rotifers	52	117	76	3	19	92
Copepoda	70	23	126	48	51	39
Do., larval	155	77	85	26	36	106
Balanus	25	9	8	20	18	59
Noctiluca	216	630	218	583	—	605
Total	539	872	542	693	—	918

In carrying out these investigations you learn, at the end of the year, how to go about the inquiry, and form certain conclusions never dreamt of before, and you find it is important to carry out other kinds of investigations, when it is too late. The result of his experience last year showed him that he had much more to learn, by carrying on his observations in a different way, and studying the relation and distribution of these minute animals which formed an important factor in the life-history of the different animals which abound in the sea. The Noctiluca and the Entomostraca must feed on minute objects, and they in turn form food for the larger animals, which again form food for the fishes. He believed that by further investigation in this direction it would be possible to throw some light upon the distribution of fishes and their migra-

tion. In conclusion, he said that in the present state of science it would be well if, instead of studying a whole host of phenomena, each took up a particular branch, and made it a special study, for by so doing they might throw some light upon that which at the present time was obscure.

OIL-IMMERSION OBJECTIVES.

ANY one who has once used the homogeneous immersion objectives, not alone for the resolution of diatoms, but for the investigation of minute pond life, will, we imagine, agree with us that the superiority of their performance over dry objectives is such as to give an impetus to their increased use. When first introduced the price was excessively high—only a favoured few having them within their reach, but thanks to Herr Leitz, of Wetzlar, we can now purchase a $\frac{1}{12}$ for £5, and a $\frac{1}{18}$ for £9.

These objectives perform very satisfactorily upon most objects, and those which have passed through our hands in the verification department have been very fair glasses.

Of course we do not mean to say that they come up to, in every particular, the high-priced lenses of Powell and Lealand, but if we had to institute a comparison we should say that to the lenses of this celebrated firm they stand in the same grade as the second series of some makers do to their first series.

The numerical aperture of these lenses is what may be termed moderate for oil immersions, and there is plenty of working distance; the $\frac{1}{18}$ has a working distance sufficient to run over all the mounted objects in our cabinet; it has an aperture of 1·26 or 112° balsam angle. In magnifying power it is more nearly a $\frac{1}{20}$ than $\frac{1}{18}$, and the collars of several of the monads were shown beautifully with it.

The $\frac{1}{15}$ we examined, singularly, had less working distance than the $\frac{1}{18}$, though it possessed exactly the same aperture, and this glass would not work over our thickest covers. The $\frac{1}{15}$ was, however, much better corrected than the $\frac{1}{18}$, and consequently would stand deeper eye-pieces. With the $\frac{1}{12}$ we have had much experience, as several of them have passed through the verification department, and we feel bound to say that the purchase of this glass is a good investment.

The $\frac{1}{12}$ has a magnifying power equal to a real $\frac{1}{13}$, and has the working distance of a Zeiss dry $\frac{1}{14}$; its aperture is 1·22 or 107° balsam angle. We have lately had an opportunity of

examining one of Powell and Lealand's high-class $\frac{1}{12}$ths (homog.). The aperture varied from "open" to "closed" 1·36 to 1·42. The working distance was ·007 inch., while the Leitz $\frac{1}{15}$, which would not work over *all* our slides, had a working distance of only ·005. Our Powell and Lealand water immersion $\frac{1}{8}$ of 1·20 numerical aperture has a working distance of ·004, and can only, therefore, be used with the thinnest covers. The advantage of oil immersion in increasing the working distance will therefore be seen. With the Leitz's $\frac{1}{12}$ homogeneous immersion we have lately spent much time. The podura scale illuminated with the immersion paraboloid shows out splendidly, while the *Amphipleura pellucida*, used with the vertical illuminator, is resolved without any difficulty. The collars of monads are well shown, and specially good are the shows of *Bacillus tuberculosis* and *B. anthracis.*

Amphipleura pellucida is a good exercise for the student, and he should not be satisfied until he is able to bring out the markings clearly. The Leitz's $\frac{1}{12}$ homogeneous is capable of doing this, but we venture to say that not one student in ten will be able to display "the hateful markings" until some one has shown him the "knack."

Illumination has more to do with bringing out the structure of objects than some microscopists imagine, and here is good exercise for them.

EVENINGS WITH THE MICROSCOPE.

I.

WE will commence by supposing every reader a beginner in microscopic studies, and to be the possessor of a microscope, an eye-piece micrometer, a common paraffin lamp, and a bull's-eye condenser. Perhaps he is also possessed of many accessories, but for the present anyhow we will dispense with these things, and only add to our stock as we progress with studies of difficulty.

A beginner nearly always is exceedingly anxious to be the possessor of some slides; well, this is only natural, and so we will indicate what his first two slides should be. It is very seldom that such slides are purchased in the first instance by the student, but if our advice be followed there is no doubt but that he will find progress in the microscopic art to be easy and rapid. These first slides which can be purchased for a few shillings should be :—

1. A stage micrometer, ruled preferably on the French and English systems, in one slide; and,

2. Abbe's test plate.

The student's microscope perhaps may be a binocular, but most probably it will be one of those working instruments illustrated on page 38 of "Practical Microscopy." We will suppose it to be fitted with an inch objective, and also with a $\frac{1}{6}$th, both of moderate angles of aperture.

The first exercise must be to measure the objectives and oculars, *i.e.*, their magnifying power, and the state of their corrections; the working distance can only be correctly measured by special appliances which need not be mentioned here yet.

The stage micrometer, when viewed with the combined objective and ocular, will show us the flatness of field or otherwise, and this should be studied on all lengths of tube from seven to eleven inches, so that by the appearance of the object, the observer may know whether, in order to produce the best effect, the tube of the instrument should be shortened or lengthened from the normal. Some objectives are very sensitive to alterations in the length of tube, and will only work well at one set length.

Let us now proceed to measure the magnifying power of our one-inch objective. For this purpose we must place the micrometer on the stage, and by preference Ramsden's eye-piece micrometer in its place at the upper extremity of the tube. The micrometer in this eye-piece is always below the field lens, and is generally $\frac{6}{10}$ of an inch in length, each $\frac{1}{10}$ being graduated into ten parts. The length of tube must now be adjusted and the stage micrometer brought to a focus, so that the distance between the two micrometers (that of the stage and of the ocular) is rather more than ten inches, the distance may be variable, but must permit of accurate measurement, say for most practical purposes to the one-tenth of an inch. If n represents the magnifying power at the distance l, the following formula will give the true denomination of the lens:

$$\text{Real focal length} = \frac{n\ l}{(n+1)^2}$$

To give an example: Suppose the one-hundredth space of the stage micrometer be magnified so that it occupies the space of twelve of the hundredth lines of the ocular micrometer, it will be clear that the amplification is twelve diameters, the distance between the two micrometers is ten inches. Multiplying these together, we get $12 \times 10 = 120$, place this as the numerator of the fraction; the number of diameters magnified must now be added to by one, and the whole squared to form the fractional denominator, in this case $(12+1)^2 = 169 \div \frac{120}{100} = \cdot 71$, or roughly $\frac{7}{10}$ of an inch.

As a guide to the student, we give the measurements of several one-inch objectives which we have examined :—

No.	n	l	$\frac{n\,l}{(n+1)^2}$
1.........	10.4	10	.80
2.........	10.9	10	.77
3.........	12.0	10	.71
4.........	9.0	10	.90
5.........	9.5	10	.86
6.........	10.0	10	.82

The student may have good practice in this process by varying the distance between the two micrometers, to the utmost limits of his instrument; he will find it excellent practice, and as a result he will be aware of the exact nature of his objectives, and better able to follow us in our remarks in subsequent chapters.

The formula here given is usually termed Cross's formula, and is a very good method for observers at a distance from each other of comparing the magnifying power of their objectives. It has been suggested that the method is not absolutely accurate, but it is sufficiently near for all practical purposes.

Eye-pieces may be measured in exactly the same manner; the student will be able with a little ingenuity to rig up a small box with an aperture at one end to receive the eye-piece, and a ground glass at the other, ruled with pencil into inches and tenths; this, of course, takes the place of the eye-piece micrometer in the former example. The eye lens of the ocular is made to point towards a rule, divided into inches and tenths, and this rule is moved backwards or forwards until a correct focus is obtained. The magnifying power can be then easily reckoned, and when the distance between the two scales is known, the calculation is just the same as for objectives. The following measurements were made by the Editor of the oculars of some of our leading firms:—

Designation.	n	l	$\frac{n\,l}{(n+1)^2}$
A...............	10	25	2.06
B...............	16	25	1.38
Ramsden's B...............	12	18	1.28
Periscopic one inch	17	19	1.00
C...............	25	24.5	.94
Kelner C...............	26.5	24.5	.85
D...............	45	25	.53
E...............	57	24.5	.41
F...............	80	24.5	.30
Tolles $\frac{1}{8}$ solid...............	250	20	.13

The student will now see how to go to work in order to arrive at a correct knowledge of the optical portion of the instrument, and if he varies these experiments as far as he is able, he will subsequently find he has not worked in vain.

We must now turn our attention to the Abbe test plate, as made by Carl Zeiss of Jena. This plate is composed of a series of ruled bands on covers of varying thickness, which are as follows:—.09, .12, .15, .18, .21, .24 millimeter. These covers are coated with a thin film of silver, and groups of lines ruled upon them; the ruled sides are then cemented with balsam to the polished slips. Each band is composed of ten lines ruled at about $\frac{1}{660}$ of an inch apart, and when viewed with a good objective presents a series of sharply defined black and white stripes, opaque and clear lines alternating at close intervals and lying absolutely in the same plane, so that no deviation can occur in the course of pencils of light transmitted through it.

A short and ready method of testing approximately any objective is recommended by Professor Abbe, as it is applicable to all instruments without requiring any apparatus except the test object already described. This may be briefly explained as follows:—

First, focus the test plate with central illuminating rays, then withdraw the eye-piece, and turn aside the mirror so as to give the utmost obliquity of illumination, which the objective under trial will admit of. This will be best determined by looking down the tube of the microscope whilst moving the mirror, and observing when the elliptic image of light reflected from it, reaches the peripheral edge of the field. As soon as this is done replace the eye-piece, and examine afresh the object plate *without altering the focus.* If the objective be perfectly corrected the groups of lines will be seen with as sharply defined edge as before, and the colours of the edges must, as before, appear only as those of the secondary spectrum in narrow and pure outline. Defective correction is revealed when this sharp definition fails, and the lines appear misty and overspread with colour, or when *an alteration of focus* is necessary to get better definition, and colours confuse the images.

A test image of this kind at once lays bare in all particulars the whole state of correction of the microscope, it being of course assumed that the observer knows how to observe, and what to look for.

We have not space enough at our command this month to show how objectives can be *critically* tested by means of this plate and special diaphragms, so therefore let this exercise stand over for our second evening.

(To be continued.)

WINDSOR AND ETON SCIENTIFIC SOCIETY.

(Extracts from the Chairman's Address.)

GENTLEMEN,—My year of office having expired it is now my duty to render to you an account of my stewardship, and to place before you a brief sketch of the state of our society at the present time, of its transactions during the past year, and of its prospects in the future. I am happy to say that at the end of this, its third year of existence, its position, both as regards the numbers of its members and the state of its finances, is as favourable as its most sanguine friends could have anticipated. I should like to remind members that we possess some excellent microscopical specimens, which are not used quite so freely as they might be. Cole's slides are beautifully prepared and mounted, and each one is accompanied with a printed description. It will answer the purpose of any one who has a microscope to take them home one by one, and master their details. We have altogether about six dozen good and well mounted specimens. An increase in this number is a great desideratum, and the committee will thankfully accept slides from any lady or gentleman having them to spare. On the 17th January we had our annual soirée in the hall of this Institute, and it was the most successful one we have yet had, both as regards the number of visitors and the display of microscopical specimens and other exhibits. The visitors numbered upwards of 300, and there were 49 microscopes on the tables, under the charge of various ladies and gentlemen. As you all know it is in contemplation to add to the accommodation of this Institute, and we trust that the committee, who have always treated us with great kindness and liberality, will grant us one more favour by setting apart for our sole use a small room which may serve for a library, laboratory, and microscopical workroom. Nothing, however, can as yet be definitely arranged about it. The committee of this society propose that in future the monthly lectures, and the proceedings of each year, with a list of the members, be published and sold at a small price, thus securing a more permanent record of our doings. Our excellent and energetic friend (Mr. Lundy) has continued his geological evenings in the Museum to a numerous and appreciative class, having altogether delivered sixteen lectures. If he or some other gentlemen would give a somewhat similar course on botany during the summer it would, I think, much further the objects of the society. During the year ten papers were read at our monthly meetings. Where so many were excellent it would be invidious to make selections. I may say they were all of a high tone, and quite equal to those of previous

years, some of them being marked by much originality; and what is more important the after discussions were in many cases well maintained. I must make one exception in mentioning the paper on Bacteria, by Mr. Thurston, of King's College, London, as it led to further important results. All who heard his lecture must have been struck with the clear and easy manner with which he treated his subject, thereby showing his thorough mastery of it. This was still further evidenced a few evenings afterwards, when he gave some practical illustrations of the way in which the bacteria could be stained and mounted for microscopical observation. Many of the members who were then present were so pleased with the happy manner in which Mr. Thurston gave his explanations that they were anxious that he should give them a short course of lectures, or rather demonstrations, on the preparation and mounting of objects generally for the microscope. He accordingly devoted four evenings to that purpose, and about eighteen ladies and gentlemen attended, and I am sure they will all agree with me that they thus easily obtained information which it would otherwise have taken them months of hard study to have acquired. I have often been asked what is the good of non-professional people using the microscope, or what is the use of our society? This *cui bono* argument is a very favourite one with those who pride themselves above all things on being what they term thoroughly practical. Now if these persons were asked their opinion about any well-known scientific person, say Darwin, for instance, they would probably acknowledge that he was a great man or a great philosopher, though I fear they would not have a very clear notion in what his greatness consisted. Had they seen him busily and patiently accumulating his almost numberless facts, such as those connected with the way in which climbing plants turn and twist, or those connected with the habits of earth worms, or those relating to the variations of plants and animals, they would probably have asked their stock question, "What is the use of them?" Now, men of culture (I do not mean those who are usually termed scientific) think that these pursuits are of the highest value. Only a short time since I heard the Dean of Windsor say from the pulpit that if Darwin had never done anything else he had at least made manifest to us how wonderfully the lower creatures were ever unconsciously carrying out the designs and wishes of the Creator. I will take lower ground, and say that if a person will take up any study, and pursue it patiently and conscientiously he will be richly rewarded by the pleasure alone which he will feel in his pursuit, to say nothing of the advantages arising from it incidentally, such as the power which always arises from the possession of knowledge. In the working of the microscope, with which we are more particularly interested, the advantages to be derived are very

great, both in a physical and mental point of view. In the first place, it greatly improves the sense of touch, for the manipulations required in the preparation and mounting of objects, and in handling the microscope generally, cannot but greatly develop the delicacy of that sense. Again, accuracy of vision is greatly improved. When a person first looks through a microscope objects appear hazy and indistinct, but after a time he sees what is pointed out to him, or what he finds written about the specimen under observation, and by degrees he is able to observe and describe things for himself till, as in the case of the accomplished microscopist, he is enabled to unravel the most complicated structures and organisms. All this time his powers of observation are being greatly strengthened, and he is undergoing a course of mental discipline which he will find of the greatest value to him in the ordinary affairs of life. I can imagine no work better calculated to make a man careful and thoughtful; he will constantly be discovering fresh facts, and he will be constantly compelled to rectify those already observed. From these considerations I think our practical friends must allow that microscopical work is attended with material advantages, though I know the thoroughly practical man is very hard indeed to convince.

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Brook & Chrystal, 11, Market-street, Manchester.

ALL matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, The Willows, Fallowfield, Manchester.

FISH HATCHING.—Professor Cossar Ewart, in the last of a series of lectures on "Fishes for Food," in the hall of the Museum of Science and Art, at Edinburgh, referred more particularly to salmon. The lecturer described Howieton hatcheries and breeding ponds, near Stirling, where provision was made for hatching 2,400,000 eggs, and where 99 per cent. survived the process. The fish in the ponds eat three horses in a week. The lecturer strongly deprecated having the hundreds of acres of water lying barren when they might

be made to yield an abundance of fish, the stock being kept up from artificial hatcheries.

EARTH WORMS.—An interesting letter in the current number of *Nature* from Dr. Henry F. Walker, of New York, gives reasons for assuming that the extremely useful creatures in question are the invariable companions of man, and are never found in regions where he has not settled. Dr. Walker states that it is well-known to settlers on virgin soils in the United States that no earth-worms are found on the first tillage of the ground. Even in the natural meadows called "beaver meadows," no earth-worms are at first seen. After settlement they are first found in the vicinity of the stable yard, then in portions of ground enriched by stable manure, and at length appear in all soils, whether cultivated or simply pastured by domesticated animals. For years Dr. Walker has been accustomed to go to Mukoka, in the Dominion, for fishing and shooting. Mukoka is still wild land unsurveyed for settlement. The frontier settlers there have told him that until a place has been inhabited by man for five years, it is useless to look for the earthworm.

COSMOLINE.—Can any reader tell me what is the composition of this American preparation?—C. A. L.

STRATENA.—This is a compound used largely by American preparers; what is it?—C. A. L.

COSMOS.—The Editor wishes to consult the French paper *Cosmos* for Oct. 30th, 1869. Can any of our readers lend this copy, or refer to any institution or library where it is to be met with in this country?

LIVERPOOL MICROSCOPICAL SOCIETY.—On Friday, April 4th, a very interesting paper was read before this Society by J. Sibley Hicks, F.R.C.S., F.L.S., F.R.M.S., on "Alternation of Generations, as exemplified in the Jelly-fish and Fluke-worm," at the conclusion of which the usual conversazione took place.

AMERICAN OBJECTIVES.—Spencer's first-class objectives are listed by Henry Chase, M.D., Geneva, N. Y., as follow:—

Focal Length.	Air Angle.	Price in Dollars.
3 inch.	13°	$20
2 "	20°	30
1 "	40°	40
$\frac{2}{3}$ "	47°	30
$\frac{1}{2}$ "	100°	50
$\frac{1}{4}$ "	135°	40
$\frac{1}{8}$ "	150°	45
$\frac{1}{15}$ "	150°	60

Mounting in Liquid Storax.—Mr. Aylward has sent us some slides mounted in the above medium, but as yet we have been unable *to see* any advantage over the usual mounts in balsam.

Nachet's Black Ground Illuminator.—If any subscriber possesses this accessory, we should be glad of the loan of it: or a detailed description would suffice.

Foot-and-Mouth Disease.—There were only 35 fresh outbreaks of foot-and-mouth disease in England last week, and of these 12 were in Lancashire and 7 in Yorkshire. In twelve other counties in which the disease exists there were no fresh outbreaks. The total number of infected places in England and Wales is now 168, a reduction of 1,327 since the beginning of the year, and there are now only 2,862 cattle affected with the disease, as compared with 36,076 on the 4th January. In Lancashire there are now 35 infected places, with 244 diseased animals upon them; in Cheshire, 3 places, 30 animals; in Derbyshire, 5 places, 32 animals. A striking improvement has taken place in Norfolk, where, since Jan. 4, the numbers have been reduced from over 300 infected places and 7,000 diseased animals to 4 places and 10 animals. In Lincolnshire there are now only 5 infected places, with 225 diseased animals, compared with 124 places and 8,000 diseased animals at the beginning of January. The Irish returns for last week are not to hand. Scotland continues free.

The Cholera Germ.—A correspondent writes, "Amongst the many productions of that prolific and suggestive writer, Thomas Ignatius Maria Foster, is a pamphlet entitled "Observations on some curious and hitherto unnoticed abnormal affections of the organs of sense and intellect," which appeared at Tunbridge Wells in 1841. At page 27 there occurs the following passage: 'During the fine evenings on Sundays and other leisure times, several scientific friends have been in the habit of assisting me in my experiments on atmospherical electricity made with the Electric Kite. In 1832, when the cholera prevailed in England, a piece of raw meat, suspended at the tail of a kite for a few hours, was found to come down in a state of putridity, and on one occasion the meat was covered with animalcula of an unknown species. Subsequent microscopic researches showed that the secretions of choleraic and other sorts of patients were infested with different animalcula. Query: may not all diseases have their peculiar insects, which, if they do not cause, yet always attend the disorder?' This brief extract will be read with interest at the present moment, when it is claimed that the cholera germ has been detected by Dr. Koch."

The Microscopical News

AND

NORTHERN MICROSCOPIST.

No. 42. *JUNE.* 1884.

EXTRACTS FROM MR. H. E. FRIPP'S TRANSLATION OF PROFESSOR ABBE'S PAPER ON THE MICROSCOPE.

Monthly Microscopical Journal, vol. xiv., page 191.

(Continued from page 124.)

THE undulation theory of light demonstrates in the phenomena of diffraction a characteristic change which material particles, according to their minuteness, effect in transmitted rays of light. This change consists, generally, in the breaking up of an incident ray into a group of rays with increased angular dispersion within the range of which periodic maxima and minima of intensity (*i.e.*, alternation of dark and light) occur. But these angular distances are for each colour proportional to its wave-length, and increase, therefore, in size from violet to red, and are also inversely proportioned to the distances between the particles in the object which cause diffraction.

This effect, which is not only such as might be theoretically predicted, but also capable of exact calculation, may be readily observed. Having placed some object of the kind in question under the microscope and got its detail in focus, the ocular must be removed and the image of the object in the open tube viewed with the naked eye, or a suitably arranged microscope of weak power ($\frac{10}{1}$ to $\frac{20}{1}$) which can be let down in the tube to the upper focal plane of the objective. The image of the mirror or whatever illuminating surface may be used will be seen as it is formed by the undiffracted rays, and surrounded by a greater or less number of secondary images in the form of impure coloured spectra, whose sequence of colours, reckoning from the primary image, is always from blue to red. Objects consisting of several systems of lines which cross each other show not only a series of diffraction images of each group in the direction of their perpendicular, but also

other additional series in the angles between the perpendicular groups. Insect scales and diatom valves exhibit these phenomena in the greatest variety.

This method of direct observation of pencils of light coming from the object enables us to determine by experiment what part is played by diffractive phenomena in forming the image of the structure in question. A suitable test-object being placed in focus, and the light being suitably regulated by diaphragms placed immediately *above* the objective, as closely as possible to its upper focal plane, for the purpose of excluding at will one or another portion of the groups of rays exhibiting diffractive effects, the image of the preparation, as formed by those rays only which were not so shut off, could be readily observed with the ordinary ocular. The immediate result of experiments carried out in this manner was as follows, it being first premised that every trial was made with very correct low-power objectives ($1\frac{1}{4}$ to $\frac{1}{4}$ inch) and corresponding weak amplification: Higher powers, an immersion lens of $\frac{1}{8}$ inch in particular being used only to control the results obtained already with coarse objects, by experiments on the finer diatoms. The preparations for all decisive trials were of such a kind that their structure was accurately known beforehand, system of lines scratched in glass, whose linear distance varied from $\frac{1}{800}$ inch to $\frac{1}{1200}$ inch; similar groups of lines ruled on silvered glass, the silver coating being immeasurably thin; groups of lines crossing each other without any difference of level were obtained by laying upon each other two glasses, the surface in contact being separately ruled.

The facts thus ascertained are—

(i.) When *all* light separated from the incident rays by diffraction was completely shut off by the diaphragm, so that the image of the preparation was formed solely by the remaining undiffracted rays, the sharpness of outline at the confines of the unequally transparent parts of the field was *not* affected, provided the opening of the diaphragm remained sufficiently large, so that no diffraction arising from the reduction of its opening should occasion any visible lowering of the "necessary amplification;" nor will the clear recognition of separate structural particles be sensibly hindered, provided that not more than 30 to 50 of such particles are found in $\frac{1}{25}$ inch.* But the more this number is exceeded, so much the more of detail disappears; so that when the fineness of detail reaches 100 parts to the millimeter (that is, when their interspace is only $\frac{1}{2500}$ inch) nothing remains visible except a homogeneous

* The definition of number is here uncertain, because the exclusion of diffracted rays, whose diffraction is slight, can only be obtained by using a diaphragm pierced with small openings.

surface whatever magnifying power be used, or whatever mode of illuminating (direct or oblique). Even a couple of lines ruled on a glass will, under the circumstances above stated, be not otherwise distinguishable than as one broader line with sharp outlines. With the most powerful immersion lens nothing at all can be seen of the markings of *Pleurosigma angulatum*, and even the coarse lines of *Hipparchia Janeira* remain unrecognizable with a power of 200. In the case of granular objects and other irregularly shaped particles, diffracted light cannot be completely separated from undiffracted light, and accordingly there is no absolute disappearance of all the particles; but such indefiniteness of image ensues that the finer particles of the preparation fuse into a homogeneous grey cloud.

(ii.) When all light is shut off, excepting a single pencil of diffracted rays, a *positive* image of the particles in the object which caused the diffraction is formed, and appears more or less brightly on the dark field, but without any detail. Ruled lines appear as uniformly clear flat stripes on a dark ground.

(iii.) But when not less than *two* separate pencils are admitted the image always shows sharply defined detail, whether it appears in the form of system of lines, or of separate fields; nor does it matter whether undiffracted light passes in with the incident cones or not: that is to say, whether the image appears on a bright or a dark field. If fresh pencils be set in operation fresh details appear, but always different, according to the degree of minuteness, or to the nature of the markings; *and this detail is not necessarily conformable either with that of the image as seen by ordinary illumination, or with the real structure of the object as known or ascertained in other ways.* In respect to this last point the following particulars are noteworthy.

(iv.) A simple series of lines will be always imaged as such when two or more illuminating pencils are set in operation, but the lines will appear doubly or trebly fine when, instead of the pencils being consecutive in order of position, one, two, or more intervening pencils are passed over. Thus a group of two lines only in the object appears as if composed of three or four separated sets. The phantom lines thus created cannot be distinguished by help of any magnification from the normal image of actual lines of double or treble fineness, either in respect to sharpness of definition or constancy of appearance, as may be shown by a conclusive experiment, in which namely, the falsely doubled image appears side by side with the image of an object actually ruled with lines of double fineness.

(v.) When two pieces of simple lattice cross each other in the same plane at any selected angle, the systems may, by suitable regulation of the admitted pencil of light, be rendered visible together or separately, and by varying the form of illumination

numerous fresh systems of lines and variously figured fields which do not exist at all as such in the object may be made to appear with equal sharpness of delineation. These new systems of lines always correspond in position and distance from each other with the possible forms in which the points of intersection of the real lines of the object may arrange themselves in equidistant series.

With a network crossing at an angle of 60°, there appears, besides several smaller systems of lines, a third set marked just as strongly as the real network of the object, and with equal distance between the lines, inclined also 60° to the others; and when the three sets are seen together there will be seen perfectly sharply defined six-sided spaces (fields) of the kind observed on *Pleurosigma angulatum*, instead of the rhombic fields. It may be added that all the appearances unconformable with the structure of known objects which are here described were observed with exactly the same focussing under which the normal image appeared well defined, and that they occurred under various combinations of objectives and oculars with regular constancy whenever the illumination was regulated in the same way.

The constant increase of *resolving* power resulting from oblique illumination, and the greater prominence of what was before visible with central illumination, is in every instance solely produced either by the entrance of diffracted rays into the larger aperture (with oblique illumination), which would otherwise not have entered into the objective on account of their greater divergence, or because diffraction pencils which were but imperfectly taken up when direct illumination was used now enter more completely and work with greater effect, whilst the direct rays are relatively less operative.

The facts here recounted appear sufficient, when taken in connection with incontestable laws of the theory of undulation, to warrant a series of most important conclusions which affect the doctrine of microscopic vision, as well as the composition and manipulation of the microscope.

Firstly, as respects the vision of objects under the microscope. Any part of a microscopic preparation which, either from its being isolated, or from its relatively large dimensions, produces no perceptible diffracted effect, is delineated in the field of the microscope as an image formed according to the usual dioptric laws of rays concentrating in a focal plane. Such an image is entirely *negative*, being dependent on an unequal transmission of light which partial *absorption* of the rays (*e.g.*, coloured rays), or divergence of the rays (from refraction), or diffraction of the rays (produced by particles of internal structure), severally occasion. *The absorption image thus produced is an unquestionable similitude of the object itself, and if correctly interpreted according to stereometric rules, admits of*

perfectly safe inferences respecting its morphologic constitution. On the other hand, *all minute structures whose elements lie so close together as to occasion noticeable diffraction phenomena will not be geometrically imaged*, that is to say, the image will not be formed, point for point, as usually described by the reunion in a focal point of pencils of light which, starting from the object, undergo various changes of direction in their entrance and passage through the objective.

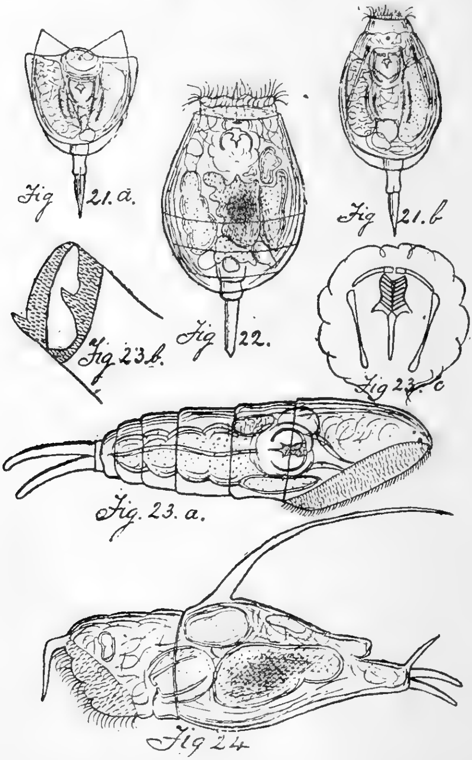
(To be continued.)

NOTES ON SOME FREE-SWIMMING ROTIFERS.

By J. E. Lord.

In my last contribution I spoke of the difficulty the student constantly experiences in his endeavour to classify some of the less-frequently recurring forms of the Rotifera. This difficulty is materially increased by the loose and irregular manner in which the different species are described. Sometimes this description will commence with the head and sometimes not. In some cases we have an elaborate description of the mastax (not always correct), while in others this important organ is not even mentioned. Occasionally the general configuration is given very fully, in precise language; in other cases, if mentioned at all, only in such vague and general terms, as to be of little or no service. Such difficulties as these are very perplexing to the earnest student, and might easily be remedied if the authorities would only exercise a little more care. It appears to me, that if a certain order was maintained, in the description of every animal, it would render the comparison of kindred forms a much more easy task. In this paper I propose to figure and describe several free-swimming Rotifers, which have puzzled me, one of which, at least, I think will be new to science. Fig. 21. This Rotifer I have procured in tolerable numbers from a well, during the whole of last summer. It is of small size and loricated. In most of its characters it agrees with the Genus Monostyla, but I am far from being satisfied that it agrees with it in that particular point from which the Genus takes its name. Pritchard says of this Genus, "Owing to the almost constant vibration of its foot-like tail, it is difficult to observe the true form of its termination, the motion producing an optical deception; hence it appears double though really single." In spite of this "warning note," I believe that the tail-feet of the Rotifers, Figs. 21 and 22, are furcate, though seldom separated. I ought perhaps to say,

that I am not aware of having seen them so separated. Characters: eye single, cervical; foot simple styliform; (?) lorica depressed, ovate, truncated anteriorly, destitute of spines; gizzard with two teeth. It is hyaline, and the lorica has four small ridges (or slits), shown in the two figures. Its lorica is too small for the animal, and is, I believe, open at the sides. Fig. 21*a*. animal retracted; Fig. 21*b*, animal exserted, showing muscles. Fig. 22. Much of the description of



Reduced from the original sketches by Photo-zincography.

the previous Rotifer is applicable to this one. It is, however, considerably larger; the lorica is not cut away anteriorly to the same extent, and it appears jointed, posteriorly. It has no spines, and its colour is reddish yellow. Its gizzard, although on the same plan, differs in its parts. I found the animal in a bottle of water and confervæ, I had dipped up in the Manchester Botanical Gardens last summer. Fig. 23. For a long time I looked upon this Rotifer as *Distemma foricula*, although the toes were not dentate

at the base. I am now convinced, however, that it belongs to another Genus of the same Family, viz., Diglena, and it seems most to resemble *D. forcipata.* Characters: cylindrical, slender (my specimens were rather stout), obliquely truncated anteriorly; toes recurved and longer than stout foot; eyes two, black; cilia, very fine, covering the oblique anterior region; the carapace seems too large for it; it is continually altering its shape, and in its general movements it much resembles Lindia, hence an old name for it was *L. vermicularis.* The Family Hydatinæa, is described as consisting only of illoricated Rotifers; however, the integument, in this particular instance, is so thick and strong as to justify the use of the term carapace. This is shown in Fig. 23*b*, which is the anterior portion of carapace or lorica, showing two spines and covered with minute tubercules. It is a very transparent Rotifer, and its beautiful and characteristic gizzard is shown (enlarged) in Fig. 23*c*. This differs, slightly, from Pritchard's figure, but it correctly represents the one I saw. Fig. 24. This Rotifer is one entirely new to me, and is of great interest, because it seems partially to fill up a gap previously existing. We have Triarthra and Polyarthra, and this one is a Monarthra, or one-spined Rotifer. It is, however, a very peculiar form, and will require more careful study than I have been able to bestow upon it. I have, as yet, only seen three specimens, and always grubbing about among decaying vegetable matter, which fact, coupled with their minuteness, renders their study one of some difficulty. Characters: obliquely truncated in front; anteriorly cylindrical for about $\frac{1}{3}$ its length; enlarging from thence to about the middle, when it gradually tapers, on the ventral side, to the toes, on the dorsal, to base of foot, where it is suddenly attenuated; foot short; toes three, about length of foot, one situated dorsally and slightly behind the other two; eyes none; having a frontal hood like Stephanops; cilia in bundles; a long tapering spine proceeds from centre of dorsal region; internal organs difficult to make out, for reasons previously stated. It has, however, a gizzard, the details of which I hope to make out when I come across another specimen. The frontal hood, for a long time, I considered merely as hooks, but on further acquaintance I am convinced that what I saw was the optical expression of a hood. I may, however, be mistaken in this, but think not. I procured them from a long ditch containing Anacharis, and a large quantity of Nitella, &c. I intended sending sketches and descriptions of two or three other less-frequently recurring forms, but must defer doing so to a subsequent issue of the Journal. These descriptions are very imperfect and most certainly are not strictly in accordance with my own opening remarks; I am not, however, "an authority," but only an enthusiastic student of nature, desirous of knowing more about the charming animals which I have made the subject

of these notes. The sketches, however, although not works of art, correctly represent the animals, as I saw them, and will enable my brother naturalists to recognise any of the animals I have so imperfectly described.

WESTERN MICROSCOPICAL CLUB.

A NIGHT WITH "BRASS AND GLASS."

THE Western Microscopical Club met in goodly numbers on Monday evening last at the house of F. Crisp, Esq., LL.B., the well-known hon. sec. of the Royal Microscopical Society. After tea and coffee, the guests adjourned to the spacious fernery and greenhouse, where, amid the illusions of ferns, lights, rocks, and fountains reflected in cunningly arranged mirrors, the routine business of the meeting was dispatched. Thence the Club proceeded to the museum, where lay the host's unrivalled collection of microscopes and microscopical apparatus, arranged tier above tier, in large ebony cabinets, around the walls and in trays along the centre of the room. All were thrown open, so that each member might himself freely take out and handle anything he desired more closely to examine. The collection is of historical interest. There were single-lens microscopes, going back to unrecorded microscopical dates. One of the earliest compound instruments with a known date was of Hooke's make in 1650. A telescopic-looking monster, some four feet long, known as "Jumbo," was confronted on the opposite side of the room by a minute though thoroughly usable microscope, of most modern construction, called the "Midget," which could boast of only four-inch height. There was also a curious Japanese microscope, made after an old Dutch model, which possessed almost all the defects that a microscope could lay claim to. In imitation of the principle of the "reflector" telescopes, the model upon which the largest telescopes have been built, a variety of forms was exhibited.

Passing from the ancient microscopes, built, most of them, on the "short and stout" principle, a collection of modern instruments, with their slim, single tubes, challenged attention. In all forms of simplicity and complexity, with ingenious devices for changing eyepieces and object-glasses, with wondrous mechanical stages and sub-stage apparatus, they fairly bewilder the spectator. In one, the polarizing prism, was slowly rotated by clock-work; in another the analyzer was whirled round some one hundred times per minute in order to produce some peculiar effects of polarization.

Turning to the binocular instruments, there were to be seen some in which not only the tube was as it were halved to form two, but the object-lens itself was actually cut in half. One was so arranged that the two half-tubes and half-lenses would fold together and form a monocular. Here were also the old specimens with parallel tubes, and the modern instruments with their converging tubes. Devices for exhibiting a series of objects arranged on a rotating plate or on a sort of water-wheel had numerous representatives. A gigantic species of brass barrel-organ, that once figured at one of the International Exhibitions at the modest price of three hundred pounds, has found a resting-place in Mr. Crisp's collection. This monster shows some five hundred objects, bringing each in turn before the gazer with its name appended.

For special work, tank microscopes, possessing a power of moving into almost any position and of stretching out to unexpected distances, were met with. For examining the throat and bladder there were instruments specially devised. One long, rod-like microscope, carrying its own electric lamp and a cold water coil to keep the lamp cool, could be passed down the throat into the stomach, so that the progress of such diseases as cancer of the stomach could be watched. Mr. Crisp sought in vain among the members for one who would swallow the apparatus in order that the remainder of the guests might survey his interior anatomy. Another microscope clasped the human tongue tightly, so that the circulation of the blood could be seen in that organ. For counting the varying number of corpuscles in the blood of the healthy and diseased, there was ready all the apparatus required. Methods of lighting—oil, gas, and electric,—diaphragms, reflectors, condensers, hot stages, electrical stages, live-cells, and all that appertain to microscopy, found there a place. The museum is a very maze of brass. Fabled Argus, could he but stroll in there, might fit himself a tube to each one of his one thousand eyes and then not gaze on empty shelves. A library containing a polyglot assemblage of microscopical literature told of the ancient and modern marvels wrought by the strange forms of brass and glass close by.

In answer to the question: Need the worker of the microscope despair because he cannot command all the wealth of ingenuity? Mr. Crisp replied by taking up a small binocular microscope, costing about ten pounds, and remarking, "That is what I should work with!" The discoveries of science have been made by means of simple inexpensive apparatus. After inspecting so unique a collection, the question naturally arises: Has the microscope attained perfection?

Many hold that it has, and that unless we get some material of higher refractive index than glass, we cannot hope to get much beyond our present power of making visible the unseen. But the

same pessimistic finality has been pronounced in every age of every art. It is true we do not know in what direction to look for any advance as regards the microscope, but none the less it may be achieved. Mr. Crisp's collection shows what has been done and is being done for the perfection of the instrument; it thus warns the inventor of the mistakes of the past, and concentrates his powers on the most productive area for further improvement.—*English Mechanic.*

THE WINDSOR AND ETON SCIENTIFIC SOCIETY.

AT the last monthly meeting of the Windsor and Eton Scientific Society, Mr. Edgar Thurston, curator of the Anatomical Museum, King's College, London, read a paper on "The Microscope, its Construction and Manipulation."

Mr. Thurston commenced by dwelling upon the importance of good illumination of microscopes, observing that if the illumination of an object was neglected the advantage of using good objectives was entirely lost. Microscopes were mainly built up according to two types, the bar movement or Ross model, and the Jackson model. Having described these, he expressed his preference for the Ross model when its workmanship was well executed. The body of a microscope, he said, with the objective, should measure ten inches, which is a very good standard, as it is the distance at which a person with normal vision holds a book so as to read comfortably, without straining the eyes. For drawing purposes, with the camera lucida, or neutral tint reflector, the standard distance from the centre of the eye-piece to the drawing surface is likewise ten inches. It is very important that the bodies of microscopes should be constructed of uniform length, as objectives, which are corrected for a certain length of body, will not work satisfactorily at any other length. An objective, for example, corrected for a body eight and a half inches in length, will not work well with a body only seven inches in length. Increase of magnifying power should not be obtained by lengthening the body by means of a draw-tube, as such a proceeding is always attended by loss of definition.

After some remarks on the eye-piece, the best form of which he said was the one in general use, Huyghenian, composed of two plano-convex lenses. A lens constructed of uniform material will not form a single white image of a white object, but a series of

images of all the colours of the spectrum, arranged at different distances. If a screen is placed anywhere in the series of images, it can only be in the right position for one colour, and every other colour will give a blurred image, and this source of confusion is called chromatic aberration. So much for the eye-piece, and I pass on to the objective, and I must confine myself mainly to the subject of immersion objectives, and the testing of objectives. The fact has been for a long time known that the quality of the image of an object depends upon the angular aperture of the objective, from the observation that certain points of detail in an object which are invisible with an objective of a certain magnifying power can be brought out by an objective which has the same magnifying power, but greater angular aperture. As a matter of fact, no amount of increase of illumination can make a dry lens equal to a wide-angled immersion lens. The advantage of wide-angled lenses consists in their greater capacity than dry lenses for picking up diffraction spectra.

Let me pass on from the somewhat complex subject of aperture to the testing of objectives, as to which the most vague ideas prevail. The popular idea is that even the highest powers can be tested without a condenser, and regardless of the kind of illumination which is employed. A great point in the testing of objectives is that the person who is testing should be in good health. You cannot properly test an objective if you have those unpleasant specks known as *muscæ volitantes* floating about in your vitreous humour. Anyone who has attempted to count the striœ of a finely-marked diatom with a wire micrometer when troubled with muscæ will be fully alive to the annoyance which may arise from them. Micro organisms (bacteria) stained and mounted in Canada balsam afford a very good test for objectives from a half-inch up to the highest powers. They should, even with full aperture of the condenser, appear sharply defined, with no appearance of milkiness, and surrounded by no spurious nebulous zone. Bacteria can be easily obtained from putrid meat or vegetable infusion, and to mount a specimen the following steps are carried out: A small portion of the fluid (the size of a pin's head) is placed in the centre of each of two cover-glasses by means of a needle, and one cover-glass superimposed on the other, so that the two surfaces of fluid are in contact with each other. The cover-glasses are then separated by pressure between the fingers, and in this way a thin layer of the fluid is obtained on each cover-glass. This is allowed to dry, and is then passed several times rapidly through the flame of a spirit lamp. A drop of staining fluid, methyl blue, methyl violet, Gentian violet, Bismarck brown, fuchsia, or other, is then dropped into the cover-glass, and after a few minutes washed off with distilled water. The specimen can then be dried by pressure between folds of

blotting paper, and mounted in Canada balsam. If the steps of the process have been properly carried out, the micro organisms will be found deeply stained.

Great attention must be paid to the illumination of your objects. It is generally accepted that the best form of natural light is that from a white-cloud, and various forms of artificial white cloud, *e.g.*, plaster of Paris, ground glass, &c., have been constructed for use with artificial light. In selecting a lamp, for working by artificial light, care must be taken to see that the chimney, if made of glass, is free from striæ and flaws. The best illumination which can be obtained is that from the most refined paraffin, and a half-inch wick. If you fail to illuminate your objectives properly, all their best qualities will be entirely lost. When I see at societies or in class-rooms, a single gas or oil lamp doing duty for two or three microscopes, I feel sorry for those who have to look at the objects displayed. He then proceeded to describe direct or central illumination, oblique illumination, and the illumination of opaque objects, and in conclusion made some remarks on the binocular microscope and the polariscope.

The simple binocular is an arrangement, whereby an exactly similar picture is thrown on to the retina of each eye, and is in reality a double monocular as no idea of solidity of form is conveyed by it. In the stereoscopic and pseudoscopic binoculars a dissimilar picture is thrown on to each retina, so that the image of objects appears in relief. The pseudoscopic binocular is of hardly any practical utility and must be regarded in the light of an optical curiosity. Even with the lowest powers binocular effects are however specially good with opaque objects, whether illuminated by the lieberkühn or other illumination. As regards the polariscope, the best form of polariser is the Nicol prism, but whatever form is used it should be large so as to admit plenty of light, and placed below the condenser in the sub-stage.

OUR SALMON FISHERIES.

THE main result of Mr. Huxley's study of the results of last year's salmon fishing, as detailed in his report as inspector of the Salmon Fisheries of England and Wales, is a confirmation of the view that we really know little or nothing about the influences which regulate the salmon supply, and which are apparently much more complex and much less due to human agency than is generally supposed. Statistical societies would be able to deduce a variety of ingenious hypotheses from the published returns, but for that

very reason Mr. Huxley is puzzled. Why, for instance, in a year of almost universal and unprecedented abundance, should the Tyne, the most prolific salmon river in the country, and one of the two rivers chosen by Mr. Huxley in his report for 1882 as illustrations of the beneficial effects of legislation, last year show a falling off instead of sharing in the general prosperity? Again, why should the takes of salmon and sea-trout be almost identical one year, diminish in almost equal ratio the following year, and then part company, the take of salmon increasing while the take of sea-trout diminishes? Mr. Huxley is equally unable to establish any consistent relation between the take of salmon and the proportion of grilse present in succeeding years, a large take being sometimes followed by scarcity and sometimes by abundance of grilse. He, of course, does not suggest any doubt as to the utility of fishery legislation. In his present report he gives an interesting history of the fishing arrangements on the Severn in order to prove the utility of legislation; but he points out that the abundance of salmon in the Severn in 1862 could not possibly have been the result of the Salmon Fishery Act of 1861, though it gave a useful *prestige* to that Act in public opinion at the outset. Every salmon river with which man interferes is, in fact, subject to two sets of conditions: the one set, due to the work of man, are conspicuous, and the other set are obscure. The futility of many well-meant attempts to improve the fisheries arises from the tendency to act as if everything depended on the regulation of the artificial conditions. Like most inspectors when they get fairly to work, Mr. Huxley appears to have developed some sympathy for manufacturers. He admits that salmon fisheries must not interfere "unnecessarily" with manufacturing and mining industries of far greater pecuniary value. Still he thinks that something may be done, and that the representatives of the various industries should pay some regard to the present and prospective value of salmon fisheries. The encouraging fact is that while it is easy to reduce a salmon river to the verge of destruction by weirs and pollutions, "Nature can, with anything like fair play, triumph over extremely serious difficulties." At present, at any rate, statistics look hopeful, the salmon take in England and Wales last year having been nearly 50 per cent. greater than in 1882, which in its turn showed an increase upon 1881. The most important work which is now in progress appears to be the providing of weirs with "passes" as opportunity serves. Some of the landowners and angling clubs object to the admission of salmon to upper waters as being likely to spoil the trout fishing, but Mr. Huxley submits that the case of the Usk affords strong evidence that valuable trout and salmon fisheries may exist together. Salmon disease did not extend to any fresh watersheds last year, with the exception of a slight outbreak in the Severn. The two points

brought out by the continued experiments of Mr. George Murray, of the British Museum, are that the fungus may attack fish with whole skins and otherwise perfectly healthy, and that an excess of lime in the water is not a predisposing cause of the disease.—*Manchester Guardian.*

AN ENGLISH BIOLOGICAL STATION.

From "The Times," 31st March, 1884.

BIOLOGICAL station, some may be inclined to think, is simply aquarium "writ large." The two certainly do coincide to some extent; a biological station, as a rule, implies an aquarium, but it includes a great deal more. In the early days of public aquaria, some twenty-five years ago, and down indeed to more recent times, attempts were made to utilise these institutions for scientific purposes, and biologists hoped that great results would follow from their establishment. It was in 1860 that the late Mr. Lloyd designed an aquarium for Paris, and two years later a similar one for Hamburg. Others soon followed both in this country and on the Continent, nearly all of them constructed on the method devised by Mr. Lloyd, and several of them under his direct superintendence. Probably the earliest on a large scale in this country was the well-known establishment at the Crystal Palace, to the management of which Mr. Lloyd succeeded on the death of Mr. J. K. Lord. Others soon followed at Brighton, Manchester, Southport, Westminster, Yarmouth, Edinburgh, Rothesay, and many other towns in this country; not to mention Vienna, Dresden, Frankfort, New York, San Francisco, Melbourne, and other places abroad, with the planning of most of which Mr. Lloyd had something to do. At the Crystal Palace, Brighton, Birmingham, and elsewhere, efforts were made to make these aquaria serve the purposes of scientific research, and at the same time to keep them open to the public as places of entertainment and some little instruction. In some of them naturalists' rooms or laboratories were established, and experiments and observations attempted with a view to adding to our scientific knowledge of the creatures whose graceful movements the public never tire of admiring. But the great essential of all such institutions was, and is, that they should pay. They were regarded by shareholders and managers as simply forming part of their big show, not to be compared in attractiveness to nigger minstrels, Lulu, or a Chinese juggler, but still useful as a bait to catch certain classes of the

public. Naturally the views and aims of the management and of the presiding naturalist clashed, and the latter had either to adapt himself to the leading purpose of the establishment, or to resign. At all events it finally became evident to biologists that science could expect little help from the ordinary aquarium, which was no more than a handmaid to the amusement of the public. To accomplish her noble purposes she must be mistress. We believe the French were the first to recognise this important truth, and to establish a station solely for the purpose of investigating the habits, organisation, and surroundings of the denizens of the ocean. Now they have quite a number of such stations in operation—as, for instance, at Roscoff, Concarneau, Villefranche, and Cette. The Austrian government maintains a similar station at Trieste, while in America the Johns Hopkins University has one at Beaufort, and Professor Alexander Agassiz another at Newport. The Dutch have for several years had a travelling laboratory erected during the summer months at different parts of their coasts. But undoubtedly the finest institution of the kind is that founded ten years ago at Naples by a German biologist, Dr. Anton Dohrn, to the work of which we have at various times referred in our columns. The Naples station is, indeed, an international institution, for although it is subsidized to the extent of £1,500 a year by the German government, its workers and much of the rest of its income, which in all amounts to about £5,000 a year, come from all parts of the world. The University of Cambridge maintains a table for one of its students, as does also the British Association. America has always one or two investigators working under Dr. Dohrn, while various European countries have their representatives. Not only has the Naples station its tanks and its laboratories, but it maintains steam launches and boats of various kinds, diving apparatus for investigating the sea bottom, dredges and trawlers, sailors and fishermen trained as collectors, and issues regularly a series of handsome Transactions, comparable to the publications of our "Challenger" expedition. The advances made in the special department of Biology connected with fishes since the establishment of the Naples station has been immense, and has had besides important bearings on other departments of the same branch of science. In this country no regular station of the kind has existed until within the last few months, when, under the auspices of the Scottish Meteorological Society, one has been established in an old quarry at Granton on the Firth of Forth, near Edinburgh. Already the naturalists at Granton have done good work in investigating the habits of the economical fishes, and especially the herring, and some of the results of their work were described to the Royal Society last Thursday by Professor Cossar Ewart, of Edinburgh. For several years the British Association has had a

committee to superintend the working of a Scottish zoological station; but the station has been peripatetic and temporary, maintained only during the summer months at different parts of the Scottish coast; nevertheless it has done excellent work. British naturalists have been long convinced that both from a scientific and economical point of view it is high time that a permanent station on the model of that of Naples were established at some suitable point on the coast of England. The success of the recent Fisheries Exhibition has encouraged this prevalent feeling, and has led our leading scientific men to take definite steps to place England in this respect on a level with other countries. As we have already announced, a meeting will be held to-day in the rooms of the Royal Society to carry out this object. This will be accomplished by founding a society having for its purpose "the establishment and maintenance of a well-equipped laboratory at a suitable point on the English coast, similar to, if not quite so extensive as, Dr. Dohrn's zoological station at Naples." Among the supporters of the movement are the most influential naturalists in the kingdom. Professor Huxley, P.R.S., will preside, and others who have promised to be present are Professor Flower, Professor Moseley, Sir Lyon Playfair, Sir John Lubbock, Professor Michael Foster, Professor Ray Lankester, Dr. Günther, Dr. W. B. Carpenter, Mr. Gwyn Jeffreys, Dr. P. L. Sclater, and Mr. W. S. Caine, M.P. (one of the Commission on Trawling). With such powerful support it seems to us that the object in view is sure to be accomplished. Both from an economical and scientific standpoint the utility, indeed the necessity, of such an establishment appears obvious. Already the Granton station has done good service to the Scottish fishermen; but even if no ends were to be served by such a station except those of pure science, these in our estimation are so important as to justify the movement which has secured such influential support. The utility in its highest, and even in its lowest, sense of encouraging scientific research may now be taken as recognised in all civilised countries. All the most valuable "practical" discoveries have been made by men who were not seeking for them, but whose sole aim was to satisfy a noble inquisitiveness. Our Government recognises the necessity of encouraging science in its magnificent establishments at Bloomsbury and South Kensington, and in its subsidy of £5,000 a year to the Royal Society for purposes of research; and none but chronic grumblers would grudge another £1,000 a year to the support of the proposed station, which indeed may be regarded as an almost indispensable adjunct to the Natural History Department at South Kensington. The necessity for research in this direction was recognised at the final meeting of the Fisheries Exhibition Commissioners, when they voted £3,000 for the

formation of a Royal Fisheries Society. They have still £2,000 in reserve, and as the exhibition was as much scientific as economic, it seems only natural that part of this should find its way to help in the construction of a station, whose sole purpose would be the investigation of the habits and organisation of the fishes of our British waters. On every side we are told that something must be done for the improvement of our fisheries; science has done so much in recent years to improve every other department of industry that, in our opinion, it is quite worth while asking her to do something for a department which is of growing economical importance. She must, however, be allowed to do it in her own way, and the names of those who are to take part in the meeting of to-day are a sufficient guarantee that any funds with which the future society will be entrusted will not be abused. The movement is one which certainly deserves public support and the countenance of the Government.

It is intended to erect the proposed laboratory at a point as rich as possible in respect of its marine fauna, and at the same time in proximity to important fishing grounds. No locality, we are told, has yet been decided, but both Torquay and Weymouth have been suggested as presenting the desired combination. There can be little doubt that Monday's meeting will be the first step to the accomplishment of the great object in view in the near future.

THE DEVELOPMENT OF THE FROG.

By Mr. Morrison.

A paper read before the Bolton Microscopical Society.

THE year, in its revolution, is ever presenting a scene of change, and to the careful observer each month opens a new arena into which he is invited to enter, there to study more fully and completely the several chapters in the great Book of Nature, as they turn up on the wheels of time.

It is not my intention to attempt laying before you one of those chapters of animated nature, but only a small fragment in this department, "The Development of the Frog."

At this time, and for several weeks to come, interesting experiments may be carried out by anyone willing to give a little time and care daily. The details of my own experiments I intend to lay before you to-night, and I would respectfully solicit your careful consideration and criticism of this subject, so as to show the truth or error of my conclusions.

Frog spawn, as you are aware, is not to be found all the year round; the time to find it is from about the first week of March till about the middle of April, according to the severity or otherwise of the weather, deposited most frequently at the shallow sides of ponds or ditches amongst the grass or aquatic plants; floating in gelatinous masses not unlike bunches of white currants, or, as some one has put it, a tapioca pudding, in which the yolks of the eggs are seen as round black specks about $\frac{1}{12}$ of an inch in diameter. When the eggs are deposited the outer or gelatinous envelope takes up a large amount of water, and swells to many times its original size into a transparent sphere about $\frac{7}{16}$ of an inch in diameter. If we examine the egg it will be seen to contain a supply of numerous little globules of air, which, besides supplying embryo tadpoles, give to the egg more buoyancy by converting it into a sort of water balloon.

Some may be disposed to ask the question, What sight of interest is to be found in the egg of a frog? My answer to this is, that the changes seen in the frog's egg are in the highest sense of the word interesting, because in these changes we are initiated into the secret of nature's first steps in its manufacture of a living form. And, further, that as far as science or research has gone, the changes seen in the egg of the frog are found to be common to the entire animal world.

In the first stage of development the yolk of the egg is seen undergoing a process of segmentation or division; this division proceeds with the utmost regularity, until, in the end, it is seen to have divided into an immense number of cells, so closely packed as to resemble a mulberry. Thus the concluding stage of all egg segmentation is aptly named the mulberry stage.

In those changes in the egg of the frog we have exhibited to the observer that which is common to animal development at large. This process of segmentation can only be followed by the destruction of an egg at regular intervals, either by crushing it between two glass slips, and examining it in water under a ¼ or ½ inch objective, or by cutting a section. By either of these processes the segmentation can be followed through all its stages. Externally the appearance of the yolk changes little till about the end of the fourth day, when it begins to assume an oval shape, until at the end of the fifth day a groove is run across this oval, dividing it into two parts.

This is a most interesting and important period in the development of the embryo, as at this time the formation of two membranes is effected, and by their development the young embryo is to be formed. This primitive groove is formed in that part of the body which is to form the back part or region. This groove is then contracted into a tube, so as to shut off the other parts

or regions of the body. And thus, at the fifth day of the egg's existence, the interesting phenomena is presented to us which indicates the formation of a nervous system in our future frog, conveying to us the broad features of structure on which all vertebrate or back-boned animals are formed. Man's own structure corresponds in its broad lines, in a most remarkable degree, with that of a frog. In this egg of the frog, then, we have a source of information full of interest to all who care to study it, and carry it to its legitimate end, when they will be able also to clear up a few points of their own existence. With the formation of this groove the walls of the body are thrown downwards, and the organs contained in them are developed. On the sixth day there is a radical change, the sand glass shape caused by the groove has given place to the distinctive outlines of a head and tail.

On the seventh day these become more marked still, until on the eighth day we have no difficulty in recognising the elementary outlines of the tadpole.

On the ninth day the progress of development is very marked. The head and tail, which were previously in a folded position on the body, begin to open out. About the end of the ninth day, if we observe carefully, we become convinced that the mysterious principle which we call life has developed itself in this animal, and that it is actually able to feebly wag its tail.

You will have observed in this division of the ovum the interesting fact has become apparent that the embryo frog, unlike birds, &c., does not originate in a single vesicle or sac on the surface of the yolk, but that the whole substance of the yolk has become assimilated or transformed into the embryo tadpole. From this stage the observer cannot fail to be impressed with the fact that a rapid change is proceeding daily. The gills, the tail, and the nostrils develop somewhat quicky, while the little animal is also seen to be daily, or rather I should say hourly, gaining in strength and activity, until at the fourteenth day it is seen to put forth all its energy in bringing its head and tail together with a jerk. These desperate efforts are interesting and instructive, because it teaches us that it is exerted to free it from the cords which bind it to the centre of its transparent sphere. At the end of a series of desperate struggles the little animal seems quite exhausted, but it gradually moves forward till it finds its way to the outside of the egg. But, like two fond friends loath to part, it takes up its abode for the next two days on the outside of the egg, and thus absorbs all that remains of the nutritious fluid inside the egg, while it occasionally indulges in a vigorous wriggle. As it gains in strength it is seen to make short excursions, thus testing its power of swimming. These excursions become more frequent, and are gradually extended in length, serving the double purpose

of developing its muscular power, and of throwing or getting rid of the black pigment matter which covers the young tadpole, and renders it very opaque. When this is thrown off it assumes a brownish grey colour, when the capillary circulation may be seen in the tail. From its emerging from the egg the circulation may be observed in the gills; when the young tadpole emerges from the egg its system is quite elementary. The head, being unusually large in proportion to the body, is split or opened a little on the under surface, by means of which the young tadpole is able to attach itself to any surrounding object. In this state it remains, as I have said, for about two days, seemingly without either eyes, ears, or mouth. On the sixteenth day a somewhat sudden change takes place. The mouth has separated with a pair of soft protruding lips, while the eyes and ears are quite visible. A groove also separates the head from the body. The tail has also expanded, and become partly transparent, and the lips assume a horny appearance.

The little animal has now developed into a perfect tadpole, and with the completion of those changes begins the interesting microscopic observation of the circulation of the blood, and the development of its internal organs. In studying the habits and development of aquatic animals the first principle to be mastered, if our experiments are to be of value and carried to a successful issue, is that we must be able to set up an aquarium on scientific or natural principles, so that the water shall remain sweet and pure for any length of time without change, by being thoroughly balanced with vegetable and animal life. In this we recognise the great fact that vegetable and animal life are co-existent, and that the one cannot exist without the other for any length of time. It is rather a remarkable fact that it is only about forty-four years since the first attempt was made to do this successfully by naturalists, and to study the habits of marine animals at home, Dr. N. B. Ward having solved the problem introduced by Sir John Dalyell in 1790, who employed men to bring up fresh supplies of sea water daily, and change the water once and twice a day in his tanks and jars. In this manner he spent largely of time and money in hopeless and ruinous failures by shutting his eyes to the simple means employed by nature in every way-side pond, where we find it furnished with an ample supply of aquatic plants. But as failures are often the stepping stones to success, so it was in this case. I am sorry to say, however, that erroneous notions still continue. It is common to hear or to see it written, "Change the water occasionally." Now the water in our ponds are not changed further than by evaporation, and an occasional drop of rain or surface water. So the water in my experiments was not changed from beginning to end, but to make up for evaporation a little water was

added occasionally so as to keep the water as near one volume as possible. The tadpoles are commonly reputed to be vegetable feeders. I beg, therefore, to ask your careful attention to the following details of experiments which I have made to test this. I obtained four separate vessels for the tadpoles of similar size and shape, containing equal quantities of water with equal quantities of water plants. The tadpoles at sixteen days from the deposit of the egg, or two days from their emerging from the egg, I put ten selected specimens in each, if they are vegetable feeders; you will observe that the conditions were all made equal. But for convenience, and that you may more readily follow this part of my subject I shall number them 1, 2, 3, and 4. I have just said that each vessel has an equal quantity of water, yet that water is very different so far as properties are concerned. No. 1 is supplied with filterings of pond water, so that with respect to animalcules it is of the richest description. No. 2 being supplied with half filterings and half pond water you will observe that in regard to animalcules this water can only be about half as rich as No. 1. No. 3 is supplied simply with pond water. No. 4 comes a step lower still, and is supplied with carefully filtered water.

We have then No. 1 supplied with filterings.
No. 2, half filterings and half pond water.
No. 3, pond water.
No. 4, filtered water.

The results of those four methods of treatment I shall now lay before you in as few words as possible, and leave the inferences to be drawn from them in your hands. I may here add, however, that the four specimens stood together on a small table, so the conditions of light and heat were also equal. The following figures are the average of four years' experiments for the complete development in each class. The denizens in these four vessels soon began to show a marked difference in their progress of development. The external gills were absorbed by No. 1 on the 21st day; No. 2, 22nd day; No. 3, 23rd day; No. 4, 25th day.

The outside gills are now replaced by internal gills developed on the gill-arches in the neck. While the tadpole continues in this stage of development its resemblance to a fish, with its inside gills, is very remarkable. The eyes of young tadpoles are, to all appearance, very rudimentary, and seem to be quite incapable of distinct vision, being covered with a blue glossy film. But No. 1 emerges out of this condition in about 30 days, while No. 4 does not get so far advanced as this until about the 40th day. The others intermediate. The eyes of the tadpoles now present a bright sparkling appearance, and by this time, with a little management in the treatment of the tadpoles, their bodies may be in such a condition that it may be said, with truth, here is an object trained

to the acme of scientific perfection. The blood may be followed through every part of the body, with the kidneys, the heart, and the gills all in view, and their connecting veins and arteries beautifully exhibited.

(To be continued.)

COLLODION AS A FIXATIVE FOR MICROSCOPICAL SECTIONS.

SECTIONS fixed by means of a solution of collodion in clove oil, as recommended by Schällibaum, may be coloured on the slide. The method is as follows: The solution, which is prepared by dissolving one part collodion in three or four parts clove oil, is applied to the slide by means of a fine brush at the time of using. The sections having been arranged, the slide is warmed for a few minutes (5—10) in the oven of a water-bath in order to evaporate the clove oil. The sections may next be freed from the imbedding mass and coloured according to desire. If the film of collodion be too thick, cloudiness is likely to arise between the sections. The cloudiness can be removed by the use of a brush, wet with clove oil, after the sections have been anhydrated by absolute alcohol.

Gage, who had begun to experiment with collodion before Schällibaum's method was published, recommends that the collodion and clove oil be applied separately: "A solution of collodion is prepared by adding to 2 grams of gun-cotton (that used by photographers is good) 54cc. of sulphuric ether and 18cc. of 95 p.c. alcohol. After the gun-cotton is entirely dissolved the solution should be filtered through filter paper or absorbent cotton. The slides are coated by pouring the collodion on one end allowing it to flow quickly over the slide and off the other end into the bottle. The prepared slides should be kept free from dust. As the collodion will not deteriorate after drying on the slide, any number of slides may be prepared at the same time. Before using a slide it should be dusted with a camel's-hair brush, and with another brush the collodionised surface of the slide should be thinly painted with clove oil. * * * * The sections are arranged as in the shellac method. The slide is warmed over an alcohol lamp and then heated in a warm chamber so as to drive off the clove oil. After cooling it may be placed in a wide-mouthed vial of turpentine, chloroform, xylol, or refined naphtha, to remove the paraffin. Naphtha is very cheap, and is the best agent we

have yet tried for dissolving the imbedding mass. The sections are usually freed from imbedding mass within half an hour, though the slide may remain in any of the solvents mentioned for two or three days, or perhaps indefinitely, without loosening the sections. When the slide is removed from the naphtha, the sections are washed with 95 p.c. alcohol by means of a medicine-dropper, or by immersing the slide in alcohol. If the sections are to be stained in Kleinenberg's hæmatoxylin or in any other stain containing 50 p.c. or more alcohol, the slide is transferred directly from the alcohol used for rinsing to the staining agent, otherwise it should be first transferred to 50 p.c. alcohol and from that to the staining agent. Whenever the sections are sufficiently stained they may be mounted in any desired mounting medium. In case Canada balsam is to be used, the slide must be immersed in alcohol to wash away the stain, and finally in 95 p.c. alcohol to completely anhydrate the sections. They are cleared with a mixture of carbolic acid 1 part, turpentine 4 parts. The balsam to be used is prepared by mixing 25 grams of pure Canada balsam with 2cc. of chloroform and 2cc. of clove oil. The latter very soon removes any cloudiness that may have appeared in the collodion film."—*American Naturalist.*

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Brook & Chrystal, 11, Market-street, Manchester.

ALL matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, **Belmont, Thorncliffe, nr. Sheffield.**

ERRATA.—Last month the printers took it into their heads to go to press without sending us a revise, several errors therefore exist in the May number :—

Page 133, 4 lines from the bottom should read :—

"in this case $(12 \times 1)^2 = 169$ and $120 \div 169 = \cdot 71$."

With reference to the article on the "Cholera Germ," on page 126, see the paragraph in our present "Notes and Queries."

Chair of Biology.—The following is from the *Athenæum*:—"A correspondent tells us that the combined chair of biology and geology in the Bristol University College is to be abolished in consequence of financial difficulties. Should this proposal be carried out, the University of Bristol will no longer be in a position to send up science candidates for the London University Examinations, while a powerful argument will be furnished to those who have opposed the extension of higher education on the basis of voluntary financial support. It is to be hoped that the governing body will yet see their way to avoid a step calculated to injure the practice and utility of the College."

Disorders of the Blood.—Dr. B. W. Richardson, F.R.S., is preparing for publication a collection of his essays, monographs, and papers on "Disorders of the blood and circulation."

Anti-vivisection.—The opponents of physiological reasearch have gained an ally in Gabriel Max, whose last painting, "Der Vivisection," is intended to enforce their contention of the anti-vivisectionists.

The Cholera Germ.—The *Lancet* corrects the statement which was widely quoted a few weeks ago to the effect that Dr. Vincent Richards had successfully inoculated pigs with Koch's cholera bacillus. It appears that Dr. Vincent Richards "has not been following in the lines of Koch; but, on the contrary, sees reason to believe that the poison of cholera is of a chemical and not a vital character, and it is from his endeavour to isolate the virus and communicate it to the lower animals that the original but erroneous report has arisen."

Manchester Microscopical Society.—The ordinary monthly meeting of the Manchester Microscopical Society was held on Thursday evening, May 1st, Mr. William Blackburn, F.R.M.S., one of the vice-presidents, presiding. A paper was read by Mr. Henry Hyde on the Epidermis of Plants and their Appendages. The paper was illustrated by prepared drawings of several varieties of the hairs of plants, and also by diagrammatic sketches on the blackboard. Mr. E. Ward, F.R.M.S., read a short paper on Volcanic Dust, illustrated by specimens, some of the finest dust being mounted as a microscropic slide for polarization. There was a capital display at the conversazione which followed the papers, many of the members having brought their microscopes, and thereby aiding Mr. Hyde and Mr. Ward in illustrating their papers. During the evening Mr. Stanley, F.R.M.S., distributed specimens of the mosses recently gathered at Llandudno.

Stockport Naturalists' Society.—The ordinary meeting of the Stockport Society of Naturalists was held on Wednesday, May—, Councillor Barber in the chair. Eight new members were elected, and a committee was appointed to organise summer excursions in connection with the society. Mr. A. Willett read a paper on Mosses, illustrated by over 100 specimens and numerous diagrams. Microscopical mounts of mosses in fruit and flower were shown by Messrs. Barber, Hudson, Bickerton, and Wakefield. Mr. F. Hudson, M.R.C.S, L.S.A., read a short communication on a young water spider, which he had carefully watched through incubation. Mr. T. Entwistle exhibited and explained a few varieties of Polyanthi.

Treasury Grant for Scientific Investigation. — The Treasury have agreed to allow £500 to the Fisheries Board of Scotland for the purpose of scientific investigation.

Manchester Field Naturalists.—The Committee of the Manchester Field Naturalists and Archæologists' Society have issued their programme for the first half of the summer excursion season. The places to be visited, beginning on Saturday next, are Morley Meadows, near Wilmslow; Mobberley and Knutsford; the Bollin Valley and Hale; Sherwood Forest and Hardwick Hall (the Whitsuntide three days' excursion); Chorlton-cum-Hardy and Barlow Moor; Biddulph Grange and Mow Cop; Kersal and Prestwich; and Miller's Dale and Chee Tor. In the middle of June there will be a gathering of the scientific societies of Manchester and the neighbourhood in the Botanical Gardens.

Manchester Natural History Society.—The usual weekly meeting of the Lower Mosley-street Society was held on Monday evening, May—. The chairman (Mr. George Burgess) showed a quantity of cultivated flowers, including Rhododendrons, Azaleas, and Auriculas; Mr. Whitehead some moths and butterflies; Mr. Dodd, flowers of fruit trees; and Mr. H. Hyde, wild plants and marine objects from Llandudno. Mr. William Forster exhibited an extensive collection of varieties of the common Polypody fern, all of them far removed from the normal type, some of the ferns being very finely divided, and extremely beautiful.

Hulme Field Naturalists.—At the last ordinary meeting Mr. H. J. Edge read a paper on some Medicinal Botanical Products. The paper was limited to a description of the various kinds of barks used in medicine. The several species of Cinchonas were shown, and other barks, including barberry, berbeern, cascarilla, cassia, mezereon, oak, augustura, elm, and buckthorn, were treated of at

length. The paper was illustrated with specimens of all the barks in the Pharmacopœia and the principal substances extracted from them.

Manchester Health Committee.—Some complaints having been made with regard to the action of some of the sanitary inspectors of the Manchester Corporation in the removal of infectious cases to hospital, the Manchester Medico-Ethical Association appointed a deputation to confer with the Health Committee on the subject. The deputation met the Health Committee on April 7, and, after some friendly discussion, it was agreed that the following suggestion of the deputation should be adopted, viz., "That in future no case of infectious disease should be removed to hospital unless such removal be certified as advisable by the practitioner in attendance."—*British Medical Journal.*

New Books on Science in the Manchester Free Library :—

Andrews (H. C.) Coloured Engravings of Heaths. Three vols. 1802-9
Birmingham Philosophical Society's Proceedings. Vol. 3. (Presented by the Society) ... 1884
Claus (C.) Traité de Zoologie ... 1884
Daniell (A.) Text-Book of the Principles of Physics ... 1884
Entomological Magazine. Five vols. ... 1833-8
Gray (A.) Measurements in Electricity and Magnetism... 1884
Hammond (R.) Electric Light in Our Homes ... 1884
Mayer (A. M.) Sport with Rod and Gun. Two vols... 1884
Regimen Sanitatis Salernitanum. Ed. by Sir A. Croke... 1830
Scientific Results of the Voyage of the Challenger. Vol 8... 1883
Tieghern (P. von) Traite de Botanique... 1884
The Naturalist. Ed. by Morris... 1851-8
Thompson (W.) Natural History of Ireland. Four vols.... 1849-51
Transactions of the Entomological Society. Ten vols... 1836-61

Catalogue of Scientific Books.—Mr. W. P. Collins has just issued No. 12 of his catalogue of microscopic books. There are many interesting works to be found in the list.

Bolton Microscopical Society.—The April meeting of the above society was a great success, the members mustering in force. The section presided over by Mr. Robert Harwood, provided the programme for the evening. The paper on the Development of the Frog, by Mr. Morrison, was well illustrated by drawings, and at the conclusion many of the members entered into an animated discussion of several points brought out in the paper. A vote of thanks having been given to Mr. Morrison, the meeting took the form of a conversazione.

NOTICE
OF REMOVAL.

Mr. GEORGE E. DAVIS begs to inform his readers that he has removed from The Willows, Fallowfield, and for the future his postal address will be:—

BELMONT,
THORNCLIFFE,
Nr. SHEFFIELD,

Where all communications should be addressed.

All parcels by carrier or rail should be directed to *CHAPELTOWN* Station, M. S. and L. Railway.

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Mr. GEORGE E. DAVIS is now prepared to undertake general microscopical investigations and to give advice on such matters.

The Microscopical News

AND

NORTHERN MICROSCOPIST.

No. 43. *JULY.* 1884.

EXTRACTS FROM MR. H. E. FRIPP'S TRANSLATION OF PROFESSOR ABBE'S PAPER ON THE MICROSCOPE.

Monthly Microscopical Journal, vol. xiv., page 191.

(Continued from page 145.)

NOW, to anyone who clearly realizes in his own mind what are the assumptions upon which a similitude between an object and its optical image is commonly accepted, the foregoing facts must suffice to lead to the conclusion that, under the circumstances above indicated, such acceptance is a purely arbitrary supposition. As a positive instance of the contrary stands the conclusion to which experiments lead by rigorous deduction, namely, that *different structures always yield the same microscopic images as soon as the difference of diffraction effect connected with them is artificially removed from the action of the microscope; and that similar structures as constantly yield different images when the diffractive effect taking place in the microscope is artificially rendered dissimilar. In other words, the images of structure arising from the operation of the diffractive process stand in no constant relation with the real constitution of the objects causing them, but rather with the diffraction phenomena themselves, which are the true causes of their formation.* As this is not the place to enter into a physical exposition of such phenomena, it may suffice to say in brief, that the conclusions here deduced from facts won by direct observation, are fully substantiated by the theory of undulation of light, which shows not only why microscopic structural detail is not imaged according to dioptric law, but also how a different process of image formation is actually brought about. It can be shown that the images of the illuminating surface, which appear in the upper focal plane of the objective (the direct image and the diffraction images), must each represent, at the point of correspondence, equal oscillation phases when each single colour is examined separately.

The delineation of structure seen in the field of the microscope is in all its characters,—those which are conformable with the real constitution of the object as well as those which are not so—nothing more than the result of this process of interference occurring where all the image-forming rays encounter each other. The relation existing between the linear distances from the axis of the microscope of constituent elements of the aperture image, and the various inclination of rays entering the objective, taken together with the dioptric analysis of the microscope, afford all the data necessary for complete demonstration of the above positions. From them may be deduced that in an achromatic objective the interference images, for all colours, coincide, and yield as a total effect achromatism, thus differing from all other known interference phenomena.

The final result of these researches may be thus stated:

Everything visible in the microscope picture which is not accounted for by the simple "absorption image," but for which the co-operation of groups of diffracted rays is needed—in fact all minute structural detail—is, as a rule, not imaged geometrically, that is, comformably with the actual constituent detail of the object itself. However constant, strongly marked, and so to speak materially visible, such indications of structure may appear, they cannot be interpreted as morphological, but only as physical characters; not as *images* of material forms, but as *signs* of certain material differences of composition of the particles composing the object. *And nothing more can be safely inferred from the microscope revelation than the presence, in the object, of such structural peculiarities as are necessary and adequate to the production of the diffraction phenomena on which the images of minute details depend.*

From this point of view it must be evident that the attempt to determine the structure of the finer kinds of diatom valves by morphological interpretation of their microscopic appearances, is based on inadmissible premises. Whether, for example, *Pleurosigma angulatum* possesses two or three sets of striæ; whether striation exists at all; whether the visible delineation is caused by isolated prominences, or depressions, &c., no microscope however perfect, no amplification however magnified, can inform us. All that can be maintained is the mere presence of conditions optically necessary for the diffraction effect which accompanies the image-forming process. So far, however, as this effect is visible in any microscope (six symmetrically disposed spectra inclined at about 65° to the direction of the undiffracted rays, ordinary direct illumination being employed), it may proceed from any structure which contains in its substance, or on its surface, optically homogeneous elements arranged with some approach to a system of equilateral triangles of $0{\cdot}48\mu$ dimensions (= circa $\frac{1}{52000}$ inch). Whatever such elements

may be—organized particles or mere differences of molecular aggregation (centres of condensed matter)—they will always present a delineation of the familiar form. All ground for assuming these elements to be depressions or prominences fails, after proof that neither the visibility of the markings nor their greater distinctions under oblique illumination has anything to do with shadow effects. The distribution of light and shade on the surface of the valve in the form of a system of hexagonal fields, is the mathematically necessary result of the interference of the seven isolated pencils of light which is caused by diffraction, whatever may be the physical condition of the object causing this diffraction: the position of the hexagonal fields, with two sides parallel to the middle ribs, has its sufficient reason in the visible disposition of the diffracted spectra towards the axis of this valve, and can be deduced by calculation without any necessity for knowing the actual structure of the object.

That the same state of things obtains in numerous instances of organic forms, the study of which belongs to the province of histology, we may learn from the instance of striated muscular fibre. The manifold changes in the characters of the images which present themselves account, to a certain extent, for the notorious discordance between the representations of different observers.

In connection with the foregoing conclusions, which have an important bearing on the scientific application of the microscope, it appears, further, that the limits of "resolving" power are determinate for every objective and for the microscope as a whole.

No particles can be resolved when they are situated so closely together that not even the first of a series of diffraction pencils produced by them can enter the objective simultaneously with the undiffracted rays. As even with immersion objectives the angular aperture cannot, by any possible means, be increased beyond the degree which would correspond, in effect, to 180° in air, it follows that whatever improvement may be effected in regard to serviceable magnifying power, the limit of resolving power cannot be stretched sensibly beyond the figure denoting the wave-length of violet rays when direct illumination is used, nor beyond half that amount when extreme oblique illumination is used. The last limit is, in point of fact, already reached by the finest lines of the Nobert plate and the finest known markings on diatom valves, as far as *seeing* is concerned. Only in the photographic copy of microscope images can resolution of detail be carried any farther.

From these facts it appears that the microscope image—excluding two cases of a similar and exceptional kind—consists, as a general rule, of *two* superimposed images, each being equally distinct in origin and character, and also capable of being separated and examined apart from each other. Of these, one is a *negative*

image, in which the several constituent parts of an object re-present themselves geometrically, by virtue of the unequal emergence of light which is caused by their mass affecting unequally the transmission of the incident rays. This image may, for shortness sake, be called the "*absorption image*," because partial absorption is the principal cause of the different amount of emergent light. It is the bearer of the "defining" power, whose amount is determined by the greater or less exactitude with which direct incident light is brought into perfect homofocal reunion. Consequently, it is always the *direct* light which "defines," no matter in what direction it arrives at the objective, *i.e.*, whether the central or peripheral zones of the objective receive it. But, independently of the "*absorption image*," all such parts of the object as contain interior structure will be imaged a second time, and this time as a *positive* image, because these parts will appear as if self-luminous, in consequence of the diffraction phenomena which they cause. Now this "diffraction image" is manifestly the bearer of "resolving" power, that is, the discriminating or separating faculty of the microscope. Its development depends, therefore, in the first and chief place upon angular aperture, in so far as this alone determines, according to rules above given, the *limits of its possible operation.* But its *actual* amount will, at the same time, depend upon the exactitude with which the partial images blend together: for it is through this last act that the detail which indicates the existence of positive structural elements in the object is rendered visible. Now, inasmuch as these isolated pencils, whose confocal reunion is the necessary condition of the formation of diffraction images, occupy different parts of the aperture, and vary constantly in position according to the character of the object and the mode of illumination: it is obvious that a perfect fusion, in *every* case, of the several diffraction images, and then an exact superposition of the resultant "diffraction image" upon the "absorption image," is only possible *when the objective is uniformly free from spherical aberration over the whole area of its aperture.*

In consequence of the onesidedness with which, in modern times, the improvement of the microscope has been directed towards the increase of angular aperture, the conditions under which abnormal appearances, and especially deceptive alterations of level are produced, occur abundantly in the new high-power objectives, as repeated experience has shown me, and I assuredly do not err in expressing my conviction that the consequences of this state of things affect to an unexpected extent the numerous questions in dispute amongst microscopists concerning the interpretation of minute structures.

Since everyone must admit that the first and most imperative claim which can be made, in the interest of scientific microscopy,

upon the performing power of the instrument is *this*—that parts which belong together in the object shall also appear as belonging together in the microscopic image, it follows that uniform correction of spherical aberration throughout the whole area of aperture must be the absolute criterion and rule of guidance in the construction of a microscope. Now, it has been shown that with a dry objective an adequate compensation of spherical aberration is, as a matter of fact, impossible when the angular aperture exceeds 110°. Hence, it must be concluded that a dry objective will be less suited for ordinary scientific use in proportion as it renders visible such finer systems of lines as exceed the limits of resolving power answering to that angle (namely, $0{\cdot}35\mu$ for oblique light). The greatest possible increase of resolving power can be obtained in a rational way only by means of immersion objectives, as these alone admit of the largest possible (*i.e.*, technically practicable angular) aperture, without contravening the very first requirement of corrected spherical aberration.*

A mode of testing which turns upon the determination of the utmost limit of "resolving power," whether tried upon a "Nobert" plate, a diatom, or an insect scale, brings into play a quite exceptional direction of rays of light into the microscope, such as is, indeed, required for this purpose by the physical condition of the problem. Theory and practice teach us that every objective which is not a total failure—however imperfect in respect to correction of spherical aberration—if its lenses be but moderately well centered, can always be made to work with *one* of its zones, *e.g.*, the outermost, if during its construction it has been tried on a similar test.

The proof that an objective can resolve very minute striæ on a diatom or Nobert's test-plate, attests, strictly speaking, nothing more than that its angular aperture answers to the calculable angle of diffraction of the interlinear distance of the striæ on the test, and that it is not so badly constructed that a sufficient correction of its outer zone is impossible. A trial of this sort offers no means of ascertaining what conditions for the correct fusion of aperture

* The dry objectives made on Abbe's calculations, founded upon the principles before explained, have only 105° to 110° of angular aperture for the highest powers, and cannot pretend therefore to compete, in resolving diatoms, &c., with objectives of much higher angle. The immersion lens is constructed with a free aperture of about 100° in water, *i.e.*, somewhat more than would correspond to 180° in air, because this is attainable without serious disadvantage. Professor Abbe is, however, convinced that even the immersion lens would not lose any of its value for ordinary scientific purposes, whilst it would be materially improved in many respects if its construction were based upon calculations for a smaller aperture, "but," he adds, "in view of this universally accepted standard of valuation, the practical optician can scarcely be expected to trouble himself about qualities of performance which would be very certainly ranked amongst those of a secondary order!"

images such an objective would present in the much more unfavourable case of the ordinary observing position. Nor can the result be considered as sufficiently characteristic even of the "resolving power" in its more general attributes.

Nor can the test of "resolving power" by direct light be estimated at a much higher value. In the neighbourhood of the limit of resolution corresponding to this form of illumination, all direct light passes through the central zone, and all diffracted light through the peripheral zone of the aperture.

From the point of view presented by the theory here propounded, another method offers itself, which, while employing the usual tests, brings directly into light the particular points which mainly influence the quality of performance during ordinary use of the microscope. If it be desired to test, in a most critical way, the conditions of exact co-operation of pencils of light which pass through every part of the aperture, there are truly no better means than those afforded by natural objects of the diatom class and insect scales, provided that the mere fact of accomplished "resolution" is not made the chief consideration, but that the exact constitution of the total image produced by the objective is studied.

The considerations adduced lead to certain rules respecting the right proportion between focal distance and angular aperture, which are opposed in many points to the hitherto prevalent practice.

Since theory demands a limitation of angular aperture of 110° for all *dry* combinations, the calculation of minutest detail accessible to such objective is readily made; and it may be shown that if "resolving" power be not unfairly exalted at the cost of the general excellence of the lens, there can be no question of detail which a practised eye would not recognize with a good amplification of from 4 to 500. Now according to the present standard of technical constructive means, such an amplification may be gained with an objective of 3 mm. (1/8 English inch), even if the attribute *good* be interpreted a little more strictly than is often done. With immersion lenses, the physical limit of "resolution," even where the angular aperture is the highest attainable, does not extend so far that an amplification of from 7 to 800 will not be fully equal to it; and this amplification would be gained with ease with a well-constructed objective of $\frac{1}{12}$ inch focal length. It may be admitted that an amplification exceeding the minimum here given as theoretically necessary, might greatly facilitate observation and render it more certain *if* the additional amplification be as correct as can be possibly made, although it would not occasion any new facts to be seen. Yet one can scarcely estimate the significance of this empty amplification far beyond the limits stated, and I therefore come to the conclusion that the scientific value of an objective whose focal

length (if a dry system) is much shorter than $\frac{1}{12}$ inch, or if an immersion system, than $\frac{1}{25}$ inch, is altogether problematical.

The actual powers of the microscope (in the strict sense of correct and useful power) are, in my opinion, exhausted at these limits, so long, that is, as no circumstances of moment are brought forward which change the bearing of present theory. There exists no microscope in which there has been seen, or will be seen, any structure which really exists in the object, and is inherent in its nature, that a normal eye cannot recognize with a sharply defining immersion lens magnifying 800 times. Reports of extraordinary performances (especially from England) of unusually high power ($\frac{1}{80}$ inch?) are not of such a character as to induce me to change my opinion and lead me into similar error, for the superiority of such lenses is said to have been proved upon objects to which the results of my observations unreservedly apply, and which are said to appear under such amplification as everyone who can understand and give an account to himself of the optical conditions of such performances must know to be wholly illusory.—From *Proceedings of the Bristol Naturalists' Society*, New Series, vol. i., part 2.

THE DEVELOPMENT OF THE FROG.

By Mr. Morrison.

A paper read before the Bolton Microscopical Society.

(Continued from page 162.)

THE hind feet protrude outside the body of No. 1 at 50 days; No. 2, 60 days; No. 3, 70 days; No. 4, 90 days; while they stand out from the body ¼ of an inch at 60, 70, 90, and 120 days respectively. At this time there is a rapid development in No. 1; at 68 days the hind legs are almost full out, and the fore feet are out ¼ of an inch. The mouth is getting enlarged in size, and the horny lips are being replaced by teeth growing from a palate. The intestines get shorter, and this forms one of the strongest points in support of its vegetable feeding, while the lungs proper, which were only rudimentary and seemingly solid, are rapidly growing and expanding. The pulmonary organs or vessels also increase in number, while the bronchial vessels are shrinking, and getting beautifully less. These changes indicate an interesting fact that the respiratory organs of our tadpole are quickly becoming those of an air breathing animal, and the body is fast shrinking into the true frog shape; the tail is being rapidly absorbed, and sending out the legs to their full length.

Development is now complete. Our fish-like tadpole has now become an amphibious animal. The metamorphosis of No. 1 tadpole is now complete in 74 days; No. 2, not until 90 days; No. 3, 118 days; whilst No. 4 only gets to the concluding stage in 180 days.

You will observe in these results a difference of 106 days against the purely vegetable feeding theory. The metamorphosis of No. 1 is thus extended by No. 4, 1½ times; and, I may add, that this has been my experience for at least seven years. But further, in support of my conclusions, I have carefully observed the ponds from which I got my spawn, and have frequently found the young frogs hopping on the grass, at the edge of the pond, at 98 days. In my experiments with No. 3 they take 118 days. You will readily see there is more chance of the tadpoles catching animalcules in a pond than in a small vessel, which amply accounts for the difference of 20 days, but with No. 2 only eight days.

I shall not trouble you with a long list of opinions as to the vegetarian habits of the tadpole, but shall adduce the following as sufficient. Mungo Ponton says:—"The tadpole in due time becomes gradually prepared for an entire change in its manner of life and outward aspects. It has to alter its mode of respiration from aquatic to atmospheric, and its diet from vegetable to animal." Dr. Andrew Wilson says:—"The tadpole possesses horny jaws by which it crops the water weeds, for although the adult frog is an insect eater the youthful frog is a strict vegetarian; and coiled up within its body we may perceive the spiral and lengthy intestine proper to the plant-eating form." Dr. Carpenter also favours the vegetarian side, although he admits they do take animalcules. After this weight of evidence it is difficult to see how I can hope to convince you of its falsity; yet any one may have clear proof that it is false by getting a few tadpoles, kill one, or a stickleback, and put in with them. They will at once give evidence that they are well adapted to devour animal food with avidity. So much so, if one happens to die there is no need to make any arrangement for a funeral, as they seem to take a particular interest in a defunct relative. Further, if one shows any signs of sickness the whole community watch the result with extreme interest, and, in addition to this, the strong ones generally give the sufferer a loving squeeze or nip, just to help forward the catastrophe which they seem to know will give them a good feed. In *Science Gossip* for March, 1880, Mr. M. H. Robson, Hon. Secretary of the North of England Microscopical Society, supports my conclusions when he says, "Light appears principally necessary as a stimulus to the growth of vegetation and production of infusoria upon which they (the tadpoles) seemed chiefly to subsist." He also names 117 days as the time taken to

effect a full development; pond water being used with the first of his specimens, the number of which was not given, and it is quite possible that eight days might separate the first from the last, being only one day under the average of four years in my experiments. I now propose, for the benefit of young members, or those who have not had any experience with tadpoles, to say a few words on the process usually called putting the tadpoles on water diet, as the majority of books give nothing but the mere statement with no details. This defective book-lore, to my certain knowledge, leads in some cases to cruel and barbarous treatment of the tadpoles until friendly death releases them from their torture; whereas, if properly treated, the animals experience no bad results. I have put some hundreds through this ordeal, and I do not remember losing one by it. In my experience the tadpole is a hardy animal, which may be reared as successfully in confinement as in its native pond in the open air. I may further add that I have satisfied myself as to the injurious effects or otherwise of this water treatment. I find the tadpoles exhibit more spirit after this than those who have not been subjected to it, although the time of their ultimate development is lengthened by one half; that is to say, if the treatment has been continued for say 14 or 20 days, the time needed for their full development will be extended by 7 or 10 days respectively. In no case should this treatment exceed 20 days, as about this time they begin to show signs of suffering, and assume a sickly appearance, and in a few days usually die. My usual time is 10 or 14 days; if I cannot get them to what I want in that time I have to be satisfied. Still, the best results will be obtained when the weather is bright and clear, as under the absorbent power of light the black pigment cells in the skin soon yield to its influence, and quickly disappear altogether. I have often observed these pigment cells elongate until two of them united, then the contents of the one would run into the other, indicating, in my opinion, one of two things: either under the influence of light this pigment matter dissolves or liquefies so as to be easily absorbed and assimilated as food, or else escapes outwards through the skin. Most observers rest content by seeing the capillary circulation in the tail. This, no doubt, is in itself a grand sight, and there may be some who imagine that more than this is impossible, and that what I have said to-night about seeing their internal organs at work, so as to recognise their change and development, and be able to follow the arterial and venereal flow of blood through every part of the body, is but an idle dream or a fond fancy. If there are any such sceptics I most respectfully ask them to carry out for themselves the following simple details, when they will receive ocular proof of the truth of my assertion.

In the first place obtain, say, a dozen tadpoles, so that no

individual animal need to be treated or examined more than once when the external gills are fully extended; put one on water diet, and each week must see another subjected to the same treatment. When the first is ready for examination note down its form and characteristics; in about a week note the development of No. 2 during that period; in another week No. 3 will come under observation; and so you will proceed until the frog is reached, having at all the intermediate stations noted its progress. That those scientific examinations may be successful sunshine and attention are indispensably necessary. Carelessness will certainly mar the results. This is a case in which the water must be changed every day, as there are no plants to absorb the carbonic acid, and give out oxygen. But before the tadpoles are put into the fresh water it must be of the same temperature as the old. If this is not done the animals suffer in health. Perhaps the easiest way of making both alike in temperture is to let the fresh stand in the same room for an hour or two. Then at intervals of three or four days I give them a treat by putting them into water rich in animalcules for two hours. This change of the water, then, at equal temperature, with plenty of light, and keeping the water clean, is the secret of success. A fortnight of this treatment, simply a process of starving to reduce the number of red corpuscles in the blood from the want of food, causes the intestines also to get empty, and partly transparent, the kidneys, heart, and lungs also partake of this partial transparency. In this state, if put into a white basin or saucer with a beam of sunlight, we shall see with the naked eye the top side of the heart oscillating; the intestines opening and contracting; the lungs heaving, the eyes rolling, and the mouth working. While in the shadow will be seen the duplicate of these movements from the under side. If they are now transferred to the zoophyte trough the sight will amply repay any trouble a thousand times over.

If at about 30 days old from the deposit of the egg you examine with a magnifying power of about 20 or 25 diameters, with the upper part of the tail and the lower part of the body in view, we see the blood in the main arteries flowing outwards to the tail, while other streams of equal volume flow inwards through the veins. Just at the lower part of the body may be seen two brown projections forming an obtuse angle. These are the kidneys, where the blood may be seen flowing in at one side and out at the other. Then, by moving the stage, leaving only the top of the kidneys in view, we have a sight which is not to be surpassed in the whole realm of nature, viz., the kidneys at work, the heart oscillating, the intestines expanding and contracting, the gills waving. We see in this view the four principal organs of the body harmoniously devoted to the getting rid of waste matter, and the

purifying of the blood. This is a sight once seen never to be forgotten, and is well calculated to arouse our admiration as we behold the harmonious working of this physiological machinery.

NOTES ON SOME FREE-SWIMMING ROTIFERS.

By J. E. Lord.

On looking over my note book, I find that only one or two Rotifers remain, about the classification of which there can be any doubt; and unfortunately, the notes and sketches of these are even more imperfect than those I have already given in the Microscopical News. Fig. 25*a*, however, is a sketch of a Rotifer, which is particularly interesting on several accounts, and I am able to give more information about this form than the remainder; but even in this case, a most important point requires to be cleared up. The following is the best description I am able to furnish:—Lorica, hyaline, depressed, oval, excised in front, rounded posteriorly; ventral plate smaller than dorsal, being cut away at the sides posteriorly; foot, long, cylindrical, jointed; toes as long as foot, only slightly tapering; Eyes, in adult none; two eyes when young; Rotatory organ—(from my sketch I should imagine this to be of the Brachionean type, but the lateral lobes representing the two wheels, are not very clearly indicated; will supply the omission on first opportunity). Water-vascular canal, but no vibratile tags; eggs large, attached to Confervæ by a protecting cover. The eyes in the embryo are red, and very distinct, but were absent in all the adult specimens I saw. The eggs I often came across, attached to threads of Confervæ, by what I at first took for bands, but which, on more careful inspection, proved to be complete coverings, the material being, I presume, chitine, from its toughness and colour, which was brown. On July 16th of last year, I had the pleasure of seeing one hatch out. Noticing a very strong ciliary movement, I watched it particularly; the head was first free, and then after a struggle the whole Rotifer emerged. For some time afterwards, it was all of a tremble, and during the whole of the process, the cilia were actively in motion. It was very diaphanous, and the mastax was well displayed. Fig. 25*a*, dorsal view of Rotifer; *b*, anterior, dorsal portion of Lorica; *c*, Posterior ventral portion; *d*, Eggs with protecting cover. Another large loricated Rotifer I am unable to make out, bears on its dorsal aspect, a considerable resemblance to Brachionus, being only a little less than that well-known animal. It is, however, not depressed anteriorly in a wedge-shaped manner,

like that Rotifer; anteriorly, it has two lateral spines, and is excised posteriorly for protrusion of tail foot. This is short and stout (not at all like the Brachionean tail foot), and is terminated by rather long decurved stout toes. The last form is one which recalls *Pleurotrocha constricta*, as it has a constriction immediately behind the head; but it is as large again as that Rotifer,—and like it very active. In its general organization, it is Hydatinean; and

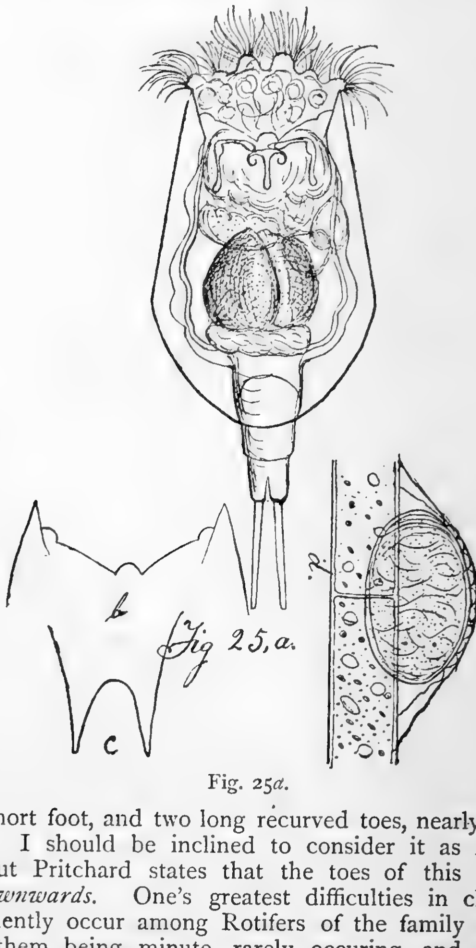

Fig. 25*a*.

having a short foot, and two long recurved toes, nearly as long as its body. I should be inclined to consider it as *Notommata Tigris;* but Pritchard states that the toes of this species are curved *downwards*. One's greatest difficulties in classification most frequently occur among Rotifers of the family Hydatinea, many of them being minute, rarely occuring, and imperfectly described in our present Text-books.

Since writing my previous papers, I have become acquainted with two facts, which have a material connection with the subject in hand. Mr. Saville Kent having accepted an appointment in Tasmania, it is highly improbable that he has any work in hand, of such importance, as one on the Rotifera, especially as it is known that he is engaged on one about Mites. The other fact is the pleasing announcement, this time authoritative, that the long expected work is actually in hand, and by a gentleman well and favourably known to most microscopists by his admirable papers on this subject. I refer to C. T. Hudson, Esq., LL.D., F.R.M.S., &c., of Clifton. This being the case nothing more need be said about the necessity of such a work; but there are one or two points about which I should like to make a few observations and suggestions. It appears to me that for several reasons, when an important work of this character is undertaken, the fact ought to be made widely known. Since the last edition of "Pritchard," 20 years ago, many new Rotifers have been discovered, and described in the Microscopical Journals, but in addition there must be a large amount of valuable material scattered about in private note books. During the last few years the microscope, as an instrument of scientific research, has been rapidly coming to the front. Microscopical societies by scores have been formed all over the country, and many of them have done good work. Judging by these signs of activity, surely it is not too much to hope that much of scientific interest; many discoveries of new Rotifers; and, we trust, many valuable additions to our knowledge of the internal organs, the habits and peculiarities, and the development of these charming animals will have been made, and only require the asking to be made of public utility. The Notes and Sketches I have contributed to the *Microscopical News* have all been written and drawn from observations made during last summer, and there can, I think, be no doubt but that others, more able than myself, would be quite willing to contribute material in order that the forthcoming work might be made more perfectly indicative of our present knowledge. Another important point is the manner and price at which it may be brought out. It is, of course, impossible to produce such a work at a price that would bring it within the reach of all. Hundreds of naturalists, who are earnestly looking forward to its production, will be unable to afford £2 or £3 for it, even if brought out in 10s. 6d. parts; but I feel assured that if it were possible to bring it out in smaller parts, say at 2s. or 3s. each, that it would be brought within the reach of great numbers, who would otherwise be unable to procure it. I have a large acquaintance among north-east Lancashire naturalists, and I know that in this matter I speak the sentiments of many who have no direct method of making them known. In conclusion, I con-

fidently leave these suggestions with those who have charge of such an important undertaking. Judging from Dr. Hudson's writings he is an enthusiastic lover of nature, and having that one touch which makes us all akin, I feel sure that he will do all that man can do to bring his work within the reach of his less favoured brethren.

MICROCOCCI OF PNEUMONIA.*

C. FRIEDLANDER has examined the micrococci contained in the alveolar exudation, and in the fluid of the lymph passages of the lungs, in cases of acute genuine pneumonia. Their presence was subsequently determined in the pneumonial fluid taken from the living patient. They were found in the greatest numbers in the pleuritic and pericardial exudations, the turbidity of these fluids often arising from enormous quantities of the micrococci. All or the greater number of these micrococci are surrounded by a more or less broad layer resembling an envelope or capsule; coloured light blue or red by gentian-violet or fuchsin respectively, and usually sharply defined externally. Sometimes each micrococcus is surrounded by an envelope of this kind of the same shape; sometimes two or three are inclosed in the same envelope; but the micrococci of pneumonia are never collected into zooglœa colonies. These envelopes are soluble in water and dilute alkalies, but insoluble in acids, and may therefore consist essentially of mucin or some similar substance.

The micrococci are best detected by placing the cover-glass with the dried-up fluid, coloured by aniline-water and gentian-violet solution, in a watch-glass with alcohol for half a minute, when the matrix rapidly loses its colour, the envelopes and micrococci much more slowly. The preparation may then be placed in a watch-glass with distilled water, examined in water, and afterwards preserved in Canada balsam or dammar lac. The envelopes are also coloured by eosin, especially by a weak solution acting for twenty-four hours; osmic acid differentiates them sharply, but without blackening them. These envelopes appear to be a highly characteristic peculiarity of the micrococci of pneumonia, never failing in acute genuine cases. They probably belong to the acme of that disease, not being found after the sixth day.

If developed by Koch's process on serum of blood and after-

* Fortschr. d. Medicin, i. (1883) pp. 715-33 (1 pl.). See Bot. Centralbl., xvii. (1884) p. 50.

wards on gelatine, with addition of infusion of flesh, peptone, and sodium chloride, the micrococci have on serum of blood the form of a greyish pellicle on the surface, and an opaque cylinder in the interior of the serum. The cultures on gelatine were especially characteristic, and were propagated for eight generations. They resembled a nail with hemispherical head, and consisted of densely crowded micrococci, usually of elliptical form, but with no envelope. They were also cultivated on potato.

Experiments were also made in inoculating the pneumonia-micrococci in animals, by injection into the right lung. With rabbits no success was obtained; while mice always died in from 18 to 28 hours. In the cavities of the pleura, partly in the fluid, partly in the lymphoid cells, were masses of micrococci, with all the characters of those of pneumonia, including the envelope. They were also found in the lungs and blood. With dogs and porpoises no result was obtained in some cases, while others were successful. Experiments were also made with mice by inhaling; when some only were infected.

The size of the micrococci and development of the envelopes differ considerably with men and other animals. Those of mice were, on the average, larger than those of man; those of porpoises were smaller, but with broader envelopes; those of dogs were scarcely larger than those of man, and the envelope comparatively narrow. The mode of preparation also has an influence on the size of the micrococci.—*J. R. M. S.*

SELECTION OF A SERIES OF OBJECTIVES.

SEVERAL writers have published their views on this subject, differing (with the exception of Dr. Carpenter) more or less from those put forward by Prof. Abbe in his paper on the "Relation of Aperture and Power."

Dr. G. E. Blackham † selects "as a set of powers sufficient for all the work of any microscopist the following:—

One 4 in. objective of 0·10 N.A. = 12° air angle nearly.
One 1 in. objective of 0·26 N.A. = 30° air angle nearly.
One 1/6 in. objective of 0·94 N.A. = 140° air angle nearly.
One 1/8 in. objective of 1·42 N.A.

The first two to be dry-working objectives without cover correction, the third to be dry-working with cover correction, and the

† Proc. Amer. Soc. Micr., 6th Ann. Meeting, 1883, pp. 33-41, 227-31.

fourth to be a homogeneous-immersion objective with cover correction, and all to be of the highest possible grade of workmanship. The stand . . . to be furnished with six eye-pieces, viz. 2 in., 1 in., and 3/4 in. Huyghenian, and 1/2, 1/3, and 1/4 in. solid. The following table shows the application of these powers to all grades of work, from that which is ordinarily done with a pocket lens to the extreme limits of microscopical vision :—

No of lines to 1 in.	N. A. required to resolve.	Equivalent angular aperture.	Amplifying power needed to give apparent size of 100 to 1 in. at 10 in.	Amplifying power actually used.	How obtained.	
					Objective.	Eye-piece.
100	Less than 0·10	Less than 10° air	None	None	Naked eye	Naked eye
500	Less than 0·10	Less than 10° air	5	12½	4 in. of 0·10 N.A.	2 in.
5,000	Less than 0·10	Less than 10° air	50	50	1 in. of 0·26 N.A.	2 in.
10,000	0·11	12° 38′ air	100	100	...	1 in.
20,000	0·21	24° 16′ ,,	200	200	...	½ in.
30,000	0·32	37° 20′ ,,	300	300	1/6in. of 0·94 N.A.	2 in.
40,000	0·41	48° 26′ ,,	400	600	...	1 in.
50,000	0·52	62° 40′ ,,	500	600	...	1 in.
60,000	0·63	78° 08′ ,,	600	600	...	1 in.
70,000	0·73	93° 48′ ,,	700	800	...	¾ in.
80,000	0·84	104° 17′ ,,	800	800	...	¾ in.
90,000	0·94	140° 16′ ,,	900	1200	...	½ in.
96,000	1·00	(180° air, 82° 17′ homogeneous imm. fluid)	960	1066	1/8 in. of 1·42 N.A.	¾ in.
100,000	1·04	86° 21′	1000	1066	...	¾ in.
110,000	1·15	About 98° ,,	1100	1600	...	½ in.
120,000	1·25	About 110° ,,	1200	1600	...	½ in.
130,000	1·35	About 125° ,,	1300	1600	...	½ in.
136,888	1·42	About 138° ,,	1368	1600	...	½ in.

. . . It has not been my purpose to lay down any single set of objectives as the only proper one, but to indicate the principles on which selection should be made, and the relation of aperture to amplifying power, and to show that there is at present no good theoretical reason for the use of objectives of greater amplifying power than the 1/8 in."

Dr. Blackham, it will be seen, advocates the use of eye-pieces as high as 1/4 in. which is largely in excess of Prof. Abbe's figures, which do not go beyond an amplification of 15 times.*

Mr. J. D. Cox believes † "Dr. Blackham has the verdict of experience with him when he says four or five lenses with a proper number of eye-pieces will cover the whole range of microscopical examination. In such a number of lenses you may get all the necessary combination of the three qualities of angle, power, and

* See this Journal, iii. (1883) p. 808.

† Proc. Amer. Soc. Micr., 6th Ann. Meeting, 1883, pp. 229-30.

working distance which you may need. Different investigators may choose different series, but no one need have a greater number in the series. Economy is to be considered in deciding whether we shall choose one or another lens; but this is also consistent with the statement that all the elements, including economy, may be combined in such a small series. The lowest glass may be anything from a 1½ in. to a 3 in. If of an angle of 20° to 25° it will have plenty of working distance and penetration. The next glass should be of 40° angle, or very near it, as this is the maximum normal angle for binocular vision of opaque objects. Its working distance should be enough to allow the use of dissecting-needles under it, and the easy illumination of dry opaque objects. These conditions are found in good glasses ranging from 1 in. to 1/2 in. objectives. The third glass should also be a dry glass, having working distance enough to accommodate work with the animalcule-cages and compressors, and upon rough histological material. Its angle should be from 100° upwards, to as wide an angle as is consistent with the necessary working distance. These conditions are found in glasses ranging from 4/10 in. objectives to 1/6 in. Beyond the three lenses thus generally described, a single immersion lens of widest possible angle seems to give all the advantages that can be attained in the present condition of the art of making objectives.

In the third and fourth of the series, the angle should be the widest consistent with the other conditions specially named, and this is the only demand of the practical microscopist in which, as it seems to me, the phrase 'wide angle' can have any appropriate place."

Dr. J. Edwards Smith* says that he has practically, for the past four years, confined himself to the use of four object glasses, namely, a 1 in. or 2/3 in. of 45° or 50°, a 1/2 in. of 38°, a 1/6 in. immersion, balsam angle ranging from, say 87° to 95°, according to the position of its collar, and a 1/10 in. immersion having a constant angle of 100°. Of the last two glasses, the 1/6 in. has a working distance of 1/50 of an inch. The 1/10 in. will work readily through covers 1/100 of an inch thick. A large amount of his work is on urinary deposits. For the examination of malignant growths and for minute pathology generally, a dry 1/4 in. of 100° is in reserve.

Mr. E. M. Nelson's † view is to give the beginner a 1½ in. and a 2/3 in.; later on a 1/6 in. may be added, and as a higher power a 1/12 in. immersion of 1·43 N.A. "For all working purposes the battery would then be complete, and the microscopist equipped to repeat any results hitherto obtained. As luxuries, a 3 in., 1/3 in., and 1/25 in. might be got. It sometimes happened that the high

* 'How to see with the Microscope,' 1880, pp. 202, 203, and 206.

† Engl. Mech., xxxix. (1884) p. 48.

initial magnifying power of the 1/25 in. enabled the observer to find some hitherto unknown object, or portion of an object, more easily than with the 1/12 in.; but when once found its details of structure would be better made out with the 1/12 in. So far it had not been possible to construct a 1/25 in. as perfectly as a 1/12 in., nor with so high an aperture; hence it would rarely bear any eye-piece beyond the lowest. The 1/12 in., however, with proper manipulation, would bear the 1 in. eye-piece, and then reveal structure that could not be made out with 1/25's, as hitherto constructed.

"Half-inch objectives had been made with apertures of 80°. Some authorities had declared that 40° was the highest aperture that could be usefully employed with that focal length. He had obtained one of the best examples of the 1/2 in. of 80°, and had made a careful series of trials with it. He had applied diaphragms above the back combination to cut down the aperture to 60° and 40° respectively, and the results might be briefly told. Taking the proboscis of the blow-fly and viewing it with the 1/2 in. diaphragmed down to 40° aperture, and arranging the illumination in the most favourable manner, he noted every detail of the picture, the sharpness and blackness of the points of the bristles, the transparency and clearness and general precision of the image; then removing the diaphragm behind the lens, he increased the aperture to 60°, and he found the image improved in every way. Increasing the aperture to the fullest extent, 80°, gave no advance upon the quality of image seen with 60° up to the 1 in. eye-piece; for this reason he concluded that 60° was the really useful aperture for a 1/2 in., and gave as much resolving power as the eye could well sustain with that combined power. No doubt the extra 20° would give the lens a higher resolving power with a stronger eye-piece, but he thought that might be better obtained with a lens of shorter focal length."

Mr. Nelson gives* the following table of apertures for object-glasses (with 1 in. eye-piece on a 10 in. tube), and says that "if ideal perfection is to be reached, the values given in the above table must be aimed at."

In.	N.A.	°
3	·08, air angle	10
2	·12, ,,	15
1½	·17, ,,	20
1	·26, ,,	30
2/3	·39, ,,	46
1/2	·52, ,,	63
4/10	·65, ,,	81
1/4	1·04, ,, water angle	103
1/5	1·3, crown glass angle	117
1/6	1·56, which has yet to be constructed.	

* Engl. Mech., xxxviii. (1883) pp. 367-8.

It will be seen that there is a wide divergence between Mr. Nelson's and Prof. Abbe's figures. For instance, for N.A. 0·65 Prof. Abbe suggests an objective of 1/8 in. and Mr. Nelson a 4/10 in.

Lastly, we may give Dr. W. B. Carpenter's views as expressed in his latest publication on the subject.*

"The 1/8 in. is (according to the writer's experience, which is confirmed by the theoretical deductions of Prof. Abbe) the lowest objective in which resolving power should be made the primary qualification,—the 1/6, 1/5, 1/4, and 4/10 in. being specially suited to kinds of biological work in which this is far less important than focal depth and dioptric precision. This view is strengthened by the very important consideration that the resolving power given by wide aperture cannot be utilized, except by a method of illumination that causes light to pass through the object at an obliquity corresponding to that at which the most divergent rays enter the objective. Now, although in the case of objects whose markings are only superficial such may not be productive of false appearances (though even this is scarcely conceivable), it must have that effect when the object is thick enough to have an internal structure; and the experience of all biological observers who have carried out the most delicate and difficult investigations is in accord, not only as to the advantage of direct illumination, but as to the deceptiveness of the appearances given by oblique, and the consequent danger of error in any inferences drawn from the latter. Thus, for example, the admirable researches of Strasburger, Fleming, Klein, and others upon the changes which take place in cell-nuclei during their subdivision can only be followed and verified (as the writer can personally testify) by examination of these objects under axial illumination, with objectives of an angle so moderate as to possess focal depth enough to follow the wonderful differentiation of component parts brought out by staining processes through their whole thickness.

The most perfect objectives for the ordinary purposes of scientific research, therefore, will be obviously those which combine exact definition and flatness of field with the widest aperture that can be given without an inconvenient reduction of working distance and loss of the degree of focal depth suitable to the work on which they are respectively to be employed. These last attributes are especially needed in the study of living and moving objects; and in the case of these, dry objectives are decidedly preferable to immersion, since the shifting of the slide which is requisite to enable the movement of the object to be followed is very apt to produce disarrangement of the interposed drop. And, owing to the solvent power which the essential oils employed for homogeneous immersion

* 'Encyclopædia Britannica,' 9th ed., xvi. (1883) pp. 269-70.

have for the ordinary cements and varnishes, such care is necessary in the use of objectives constructed to work with them, as can only be given when the observer desires to make a very minute and critical examination of a securely mounted object."

A table is then given which in addition to the magnifying-powers of objectives with the A and B eye-pieces also "specifies the angle of aperture which, in the writer's judgment, is most suitable for each. He has the satisfaction of finding that his opinions on this latter point, which are based on long experience in the microscopic study of a wider range of animal and vegetable objects than has fallen within the purview of most of his contemporaries, are in accordance with the conclusions drawn by Professor Abbe from his profound investigations into the theory of microscopic vision, which have been carried into practical accomplishment in the excellent productions of Mr. Zeiss." An extract from the table will be found on the next page.

"For ordinary biological work, the 1/8, 1/10, and 1/12 objectives, with angles of from 100° to 200°, will be found to answer extremely well if constructed on the water-immersion system."

Focal Length.	Angular Aperture.	Focal Length.	Angular Aperture.
in.	°	in.	°
4	9	1/4	50-80
3	12	1/5	95
2	15	1/6	110
1½	20	1/8	140
1	30	1/10	150
2/3	40	1/12	160
1/2	45	1/16	170
4/10	70		

"It must be understood that there is no intention in these remarks to undervalue the efforts which have been perseveringly made by the ablest constructors of microscopic objectives in the direction of enlargement of aperture. For these efforts, besides increasing the resolving-power of the instrument, have done the great service of producing a vast improvement in the quality of those objectives of moderate aperture which are most valuable to the scientific biologist; and the microscopist who wishes his *armamentum* to be complete will provide himself with objectives of those different qualities as well as different powers which shall best suit his particular requirements."—*J. R. M. S.*

LIVERPOOL MICROSCOPICAL SOCIETY.

AT the last meeting of the Microscopical Society of Liverpool, held at the Royal Institution, Colquitt-street, Mr. J. D. Siddall, of Chester, gave an address on "The Microscopical Examination of Milk and Drinking Water." The remarks offered had reference chiefly to the impurities present in the settlings or sediment obtained by allowing the milk or the water to stand undisturbed sufficiently long for all the heavier foreign matter contained in them to deposit, or by careful sieving and filtration. In the case of the milk, of which many samples obtained from different localities at various periods of the year had been examined, the sedimentary matter was found to consist almost entirely of the highly objectionable substance—cow-dung. This was present in greater or less quantity in every sample of milk examined. Milk containing large quantities of it had been observed to go bad sooner than when little was present, and several distinct outbreaks of diarrhœa and sickness had apparently been directly caused by drinking such milk. When separated from the milk, in a few hours' time the sedimentary matters teemed with Bacteria and kindred organisms, and emitted a most offensive odour. Slips of glass, which had been used for examining some of this sediment, were allowed to dry, and kept dry for several months, then they were moistened with distilled water, and it was found that the Bacteria, etc., had not lost their vitality; an hour or two's soaking sufficing to re-establish their living activity. From these, and the other observations made, it seemed manifest that the presence of cow-dung in milk was highly objectionable, and perhaps fraught with grave danger. Every possible care should, therefore, be taken to prevent any such contamination of so valuable a food. Respecting water, the purpose had been chiefly to determine the microscopical condition of the Chester Water Supply, every facility for the examination of which having been most readily given by the Water Company. The source of the supply is the River Dee, at a point about a mile above the city proper. Thence the water is conveyed in iron pipes, and caused to flow over filtering beds, consisting of about four feet of sand, and gravel, and rough stones. After percolation through these, the water is received into a large underground reservoir, and pumped thence to a high level reservoir, from which it flows by gravitation over the city. The daily consumption being about one and a-half million gallons. The quality of the river water, prior to filtration, varies considerably. At times very clear, at others turbid and discoloured through freshets or tidal influence. Above the source of supply the sewage of several towns and

villages is poured into the river. The sewage of Chester, and the refuse of numerous works and factories, also run into it below the weir at Chester. So it is obvious that water obtained from such a source can only be pronounced safe for drinking purposes if all the sewage and other organic impurities be eliminated by most careful and perfect filtration. In spite, however, of the amount of pollution thus indicated it is very often possible to see the river bed quite clearly in parts where the water is only a few feet deep. Although occasionally so clear, there is always a most strongly-marked difference in the transparency of the water before and after filtration through the filter-beds of the works. A column of the filtered water, one yard long, is clear enough to read through, whilst such a column of unfiltered water entirely obscures the view. As might be expected a microscopical examination of the sediment obtained from the top of the filter-beds, or of the river water at the source of supply, reveals contamination of the water with what has been termed "household refuse," in addition to the ordinary aquatic animal and vegetable organisms. All of these constituents are still present, but in vastly inferior quantities in the water after filtration. The utmost pains are taken by the water company to secure *perfect* filtration, and their efforts are successful in a very high degree, so much so, indeed, that the water now supplied to Chester is probably as bright, clear, tasteless, and good as any in the kingdom. Still the incontrovertible fact remains that *some* particles of appreciable size and recognisable form pass through the filter-beds and find their way into circulation. A simple microscopical examintion therefore plainly suggests that no watercourse from which water is obtained for drinking purposes should ever be permitted to receive sewage, as if once in, no system of filtration seems competent to remove it. As with milk so with water,—the obvious teaching of the whole matter is, rigorously to guard against the introduction of any excrementitious impurities into either of them. It may cost money to bring about the needful reforms in both cases. But men's lives are at stake, and they surely are not to be estimated at a money value. Great dependence is sometimes—in fact by most people—placed on the purifying power of domestic filters. Many of them are very good, some are very bad; but a very simple experiment will prove to anyone that the best of them is not capable of removing the more minute germs of certain organisms. Draw say a quart of water from the filter, seal it up in a white glass bottle the cleanliness of which has been properly assured, place the bottle in direct sunlight for a week or fortnight, and the germs which have defied filtration will multiply so rapidly as to become visible even to the naked eye. If, therefore, the germs of Algæ &c., pass through the filter, so also with others, perhaps more dangerous. A filter should only be looked upon as a sieve. Many

of them are so constructed that this sieve cannot be cleaned. Such a filter rapidly becomes a foul mass of dead and decaying matter through and over which every drop of water must be passed before some people will drink it. The consequences may be better imagined than described. It is not unfrequently urged that a microscope in the hands of an ordinary observer is just so much power allowed to run to waste. If, however, the present remarks lead to the temporary diversion of the microscopic power represented by this Society from the very natural and by no means useless study of things beautiful to things merely useful, something will have been done to remove the stigma, and results be attained, the value of which it is scarcely possible to overestimate.

We cannot agree with all the opinions expressed in this paper, and shall refer to it again in a future issue—ED.

THE RELATION OF APERTURE AND POWER IN THE MICROSCOPE.

BY PROFESSOR E. ABBE, HON. F.R.M.S.

(Read 14th June, 1882).

II.—The Rational Balance of Aperture and Power.

(*ii.*) *Division of the Entire Process of the Microscope between Ocular and Objective.*

HAVING determined—as definitely as the circumstances will permit—what total power of the *Microscope* is necessary or useful for the utilization of a given aperture, the next question can now be discussed, which is: What power of the *objective* is required for the same purpose?

From the principles on which the former discussion was based, this question has raised a distinct issue. If we find that with an aperture of 0·50 (60°) a total amplification of 265 diameters is required, in order to display the smallest dimensions which are within its reach under a visual angle of 2′, it follows that for the actual effectiveness of that amplification the microscope-system (objective and ocular combined), must so collect the rays in the ultimate image that the image-points shall have sufficient sharpness for the distinct exhibition of details of that small visual angle. The question will therefore be: What composition of the microscope must be used, and in particular, what power or focal length of the objective is necessary and sufficient, in order to obtain these 265

diameters without an obvious loss of sharpness of the image? If we are able to determine that focal length, we have at the same time assigned the *proper* focal length for the aperture of 0·50.

1. The discussion of this subject is based on the following optical principles :—

(1) If we could obtain lenses or systems of an ideal perfection, collecting *all* rays to mathematically sharp points without any aberrations, the composition of the whole microscope would be absolutely unimportant. If the effect of the aberrations is disregarded, *all* functions of the microscope depend solely on the aperture and the focal length of the *entire* system, and are quite independent of the number and arrangement of its constituent elements. Upon this assumption a given short focal length of the whole microscope, which means high linear amplification of the ultimate image (which is the quotient of the distance of vision by the focal length) could therefore be obtained just as well by means of a strong ocular at the upper end of the tube, as at the lower end by means of a strong objective. The only reason why a difference of division is of importance is the *accumulation of the effects of faults and aberrations of the lenses in the ultimate image of the microscope.* In order to prevent an obnoxious accumulation, and for no other reason, we are confined to certain limits in the distribution of the total power as between objective and ocular.

(2) The ocular is practically unimportant under the actual conditions of the microscope, so far as the sharpness of the image at the *central* portion of the field is concerned—the quality of the field outside the axis being disregarded. The length of the tube being always a considerable multiple of the clear diameter of the objective, the pencils of light are contracted to very small angles at their entrance into the ocular. The numerical computation of the spherical and chromatic aberrations originating in similar pencils, in the case of ordinary Huyghenian or Ramsden oculars, shows at once that in the neighbourhood of the axis their amount is utterly inconsiderable in comparison with the residuary aberrations of the most perfect objective. Consequently the axial aberrations which are inherent in the *objective*-image, can neither be increased nor diminished by any kind of ocular; they are enlarged only for the eye in the same ratio as the image itself is enlarged. Other properties of the image (outside the axis)—flatness of the field, uniform amplification, &c.—which *are* influenced by the ocular, are doubtless of practical importance in the use of the instrument, but they do not touch the essential points, whether a given degree of sharpness and distinctness is reached with a given power. For it makes no difference, in regard to this question, whether the available field of maximum excellence is somewhat greater or somewhat less. The interference of gross defects

of workmanship being, of course, disregarded, the ocular may always be considered as being unimportant except as a means of enlarging the objective-image; and all further discussion may, therefore, be confined to the circumstances on which the sharpness of that image which is projected by the *objective* depends.

(3) In objectives two different kinds of faults and aberrations must be distinguished. There are, firstly, *accidental* defects, arising from coarse errors of figure and want of centering of the lenses, or from the use of an unsuitable formula, or from temporary derangement of the corrections, as when the cover-glass is too thick or too thin, or the image is projected to a distance other than that for which the system was corrected. Defects of this kind can always be avoided by careful construction and proper management, and are, therefore, beside the question before us. Secondly, we have *essential* defects in the performance of objectives; the accumulated influence of certain slight imperfections in the technical work of the lenses, and certain residuary aberrations which cannot be eliminated by the most skilful construction under the actual conditions of optical work at the present time. These alone can claim a general signification, and admit of an approximate estimation according to the existing standard of optical art.

In such an estimation we do not need any detailed analysis of the various sources of defective performance. For our present purpose it is quite sufficient to enunciate certain optical propositions, by means of which the problem may be reduced to *one* question, to be answered on the grounds of practical observation.

It may be easily shown, on well-established principles, that with one and the same objective the total effect of all essential abrerrations, if measured by the *linear* diameter of the dissipation-circles in the image, always varies *in direct proportion to the linear amplification of that image*, provided the distance to which the image is projected is a considerable multiple (not less than about the tenfold) of the clear opening of the objective. This holds good (with the restriction just named) for every position of the image, and whether this is changed from a real image to a virtual image, and *vice versâ*—that is to say, that if the linear amplification is increased in the proportion of $1 : n$ by projecting the image to a greater distance from the objective, the dissipation-circles which appear instead of sharp points are always increased in the same proportion, if the accidental aberrations attendant upon the change of the conjugate foci are eliminated. This latter condition means that if an objective has its *best* correction for a certain distance (A) from the back of the objective, and the image is now projected to another distance, n A, on the same side (or on the opposite side, the image being virtual in the latter case), the correction will probably be largely deranged by the alteration, and a large amount of

new aberrations introduced thereby. But if this is properly compensated by any of the ordinary means, and the *best* correction for the new position of the image is obtained, the residuary aberration will be reduced to an amount which will exactly correspond with the change in the amplification according to the above rule.*

This statement leads now to several inferences of practical importance, which are:—

(*a*) The total effect of the aberrations (therein including the strictly residuary aberration, as well as the irregular dissipation of the rays in consequence of technical faults of the lenses) in the ultimate image of the entire microscope is, *with every given objective*, always proportional to *the total amplification of the image*, and does *not* depend on the length of the tube alone, or the depth of the ocular alone, with which that amplification may be obtained. This is easily seen if it is borne in mind that the ocular merely effects an enlargement of the objective-image, together with the dissipation-circles which are inherent therein. For if a certain total amplification N—say 500 diameters—is obtained with the whole microscope, the objective amplifying the object by N' diameters, and the ocular amplifying the objective image by N'' (say 50 and 10 respectively), then will $N' N'' = N$, and the linear diameter of the dissipation-circles in the *ultimate* image will be $N''. \epsilon$, if ϵ denote the diameter of the dissipation-circles in the objective-image. If now the same total amplification N should be obtained with the same objective by means of a longer tube and a lower eye-piece, N' will be increased (say to 100), and in the *same* proportion ϵ also, but N'' will be diminished in the *inverse* ratio (to 5). The product $N'' \epsilon$ therefore retains its former value. But if, on the other hand, the total amplification N should be increased (either by increasing the length of the tube, and therefore the value of N', or by increasing the amplification of the ocular N''), the product $N'' \epsilon$ will vary in the ratio of N, because in the one case the second factor, and in the other case the first factor, are increased in that ratio.

(To be continued.)

A biographical dictionary of physicians, edited by Dr. Hirsch of Berlin and Dr. A. Wernich, is to be published by Urban and Schwarzenberg.

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* The tacit assumption which is implied in the proposition that the compensation for change of the conjugate foci is always possible, without introducing new aberrations and without altering the new focal length and the aperture, may be readily shown to be true under the restrictions in regard to the distance of the images which have been indicated above.

MICROSCOPICAL SEPARATION OF WHEAT AND RYE-MEAL.*

L. WITTMACK records the following observations on the microscopical distinctions between wheat-meal and rye-meal. The amount of starch gives no certain character, and the size of the starch-grains is not in itself sufficient; the maximum size of the starch-grains of rye is 42–52 μ; of wheat 28–35 μ. Better characters are obtained from the grains themselves.

The denseness of the pitting in the transverse cells of wheat gives them a chaplet-like appearance. The shell or pericarp is often difficult to meet with in the finer kinds of meal.

	Wheat.	Rye.
	μ	μ
Skin, average thickness	43-50	31-40
Epidermal cells of the skin or pericarp—length	116-160	136-400
,, ,, ,, breadth	20-28	26-62
,, ,, porous pitting	very close	less close
Sub-epidermal transverse-cells—length ...	114-192	72-90
,, ,, breadth ...	14-17	11-14
,, ,, thickness of walls	5·8-8·7	3·3-5·0
,, ,, pitting ...	very dense and conspicuous	less dense, inconspicuous
Starch-cells—average larger diameter ...	56-72	40-64
,, shorter diameter ..	32-40	24-40
Iso-diametrical starch-cells—diameter ...	40-48	32-36

Good characters are also obtained from the hairs. Before grinding, both ends are removed from the grain; the embryo-end on account of the oil which it contains; the opposite apex on account of its hairs; but a few hairs are still to be found in the meal, which give the following characteristics:—

	Wheat.	Rye.
	μ	μ
Length of hairs	120-742	50-420
Diameter of the largest ...	15-21	9-17
,, at the bulbous base.	28	23
,, of the smallest ...	9-10	8
,, at the bulbous base.	14	11-14
Thickness of wall of the hair..	7	3-4
Breadth of cell-cavity ...	1·4-2 (rarely 5)	7

Wheat has therefore thick-walled hairs with narrow cell-cavity; rye thin-walled hairs with wide cell-cavity.

V. Berthold† confirms in the main Wittmack's statement. He

* SB. Bot. Ver. Prov. Brandenburg, xxiv. (1882). See Bot. Centralbl., xiii. (1882) p. 91.

† Zeitschr. f. Landwirthsch. Gew., &c., 1883, pp. 1-3. See Bot. Centralbl, xiv. (1883) p. 247.

finds the gum-cells of wheat to be decidedly larger than those of rye; the former measuring 3, the latter 1·5–2 μ. The thickness of the wall of the hairs of wheat he states at 5–8, of rye 3–6 μ; the diameter of the cavity of the former 1·5–4, of the latter 4–12 μ. —*J. R. M. S.*

MICROSCOPICAL EXAMINATIONS OF ARTICLES OF COMMERCE.*

A. TOMASCHEK points out the value of microscopical examination in the determination of the purity of many articles of commerce, and gives the following illustrations:—

Tea-leaves are readily recognized by their peculiar idioblasts.

Barley-meal is very well charterized by the beautiful tabular cells with thick wavy margins belonging to the paleæ which are always found in the meal in consequence of the close adherence of the paleæ to the fruit. The following method is recommended for their detection:—A drop of concentrated hydrochloric acid is thrown on to the meal, and rolled in it. A piece of the dough thus obtained is placed on the slide, and another drop of hydrochloric acid run on to it before covering with the cover-glass, and the cover-glass then pushed lightly backwards and forwards. The tabular cells are not only not attacked by the acid, but are coloured by it a bright sulphur-yellow colour. They may be detected even after the baking of the barley-meal.

The microscopical appearance of wheat-meal is distinguished by the peculiar properties of the paste, which can be best demonstrated in the following way:—A thin layer of meal is placed on the slide, carefully covered with a cover-glass, and then moistened by a drop of water placed on its margin. The cover-glass is then lightly pressed, and pushed backwards and forwards, the gelatinous substance being thus separated from the starch-grains, and appearing in the form of dense clouds. If glycerin is used it solidifies into bluntly angular granules, averaging 0·08–0·01 mm. in length. In order to obtain the iodine reaction characteristic of a nitrogenous substance, a comparatively large quantity of the reagent must be used, as the golden-yellow reaction of the proteinaceous substance does not appear until the starch-grains have absorbed what iodine they require. This gelatinous substance is especially well recognized by its reaction with cochineal. If cochineal-powder is scattered over the wheat-meal, and moistened merely by breathing on it, the proteinaceous masses at once take a beautiful carmine-red colour, the starch-grains remaining quite colourless.—*J. R. M. S.*

* Verhandl. Naturf. Ver. Brünn, xix. (1881) p. 15. See Bot. Centralbl., xi. (1882) p. 318.

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

No. 44. *AUGUST.* 1884.

AMERICAN SOCIETY OF MICROSCOPISTS, SESSION OF 1884.

WE have received a circular respecting the next (seventh) annual meeting of this live society from the secretary, D. S. Kellicott, and publish below that portion which is of general interest. The local committee is a large and strong one. We notice in the list many familiar names.

"The Seventh Annual Meeting of the Society will be held at Rochester, New York, beginning on Tuesday, August 19th, 1884, lasting four days. Members of the society will need little urging to attend, for the steadily growing interest in the meetings for six years is a sufficient guarantee that they will look forward to this one with eager anticipation.

The value of the organisation has been established, and we are full of hopeful expectation that all the working microscopists of the country will join its membership, and make it the centre of active microscopical investigation, and the means of mutual stimulus to better and higher scientific work. This year's meeting offers some special inducements to attend its sessions.

The time is set a week before the meeting of the British Association at Montreal, that of the American Association at Philadelphia occurring one week later still. We hope that we shall have the pleasure of welcoming distinguished men of science from the British Islands and from Canada, whose names are familiar to us from their valuable work with the microscope.

The arrangements made by the local committees are such as to ensure most agreeable and interesting sessions, with the most ample facilities for those who present papers to illustrate them by projection apparatus and otherwise.

The sessions for illustration of practical work in preparing and mounting objects, which proved so fascinating and useful a feature of the Chicago meeting, will be still more varied and instructive than before.

Titles and abstracts of papers should be sent as soon as practicable to the secretary, Professor D. S. Kellicott, Ph.D., 119, Fourteenth-street, Buffalo, New York; and all who intend to be present or to join the society are requested also to notify him or the local committee at Rochester."

J. D. Cox, *President.*

THE PREPARATION AND MOUNTING OF FORAMINIFERA WITH DESCRIPTION OF A NEW SLIDE FOR OPAQUE OBJECTS.

By F. M. Hamlin, M.D., Auburn, N. Y.

NEARLY all of the Foraminifera are in condition to mount as found, but the principal difficulty is to obtain them in sufficient quantities, for mounting, free from the sand and other materials with which they are gathered. If in sand which is silicious, they may be quite easily separated by drying the whole mass thoroughly, and then pouring on water. The sand sinks readily, but the shells being filled with air float upon the surface of the water and may be skimmed off; or a small quantity of the sand may be placed in a watch glass and subjected to a whirling, shaking motion when the shells will work out on the top and may be picked off.

These plans do very well for the coarse forms, but for the finest, and for calcareous sands, such as the famed Bermuda sand, there is no plan so satisfactory as to search through the material with the microscope.

To save time and labour I separate the sand into grades by passing it through sieves of different degrees of fineness. Three grades are sufficient, one coarse, medium and fine. The shells from the last will exercise the skill nearly as much as diatoms. Having sifted the sand I examine it on a specially devised slide, made as follows: A piece of pasteboard the size of an ordinary slide has a long slit cut in it and is then fastened to a glass slide. The width of this slit is of importance, and is determined thus,—take a low power objective, say a three or four inch, which affords just sufficient power to see the shells well, and measure the width of its field. Make the slit, or opening in the pasteboard just twice this distance. The slide being ready, a little pinch of sand is put on the glass and a slight shake spreads it out in a single layer confined by the pasteboard. It is then placed under the microscope and moving it so that the edge of the pasteboard is just visible, pass up on one side and down the other, and every particle of the sand is brought into

view without loss of time in searching over the same portions many times, and perhaps entirely omitting others. It is surprising what a quantity of sand can thus be looked over in a short time, by this systematized labour.

Supposing the shells are clearly seen among the sand, how are we to get them? There is no royal road that I know of, they must be picked out singly. I use for this purpose a very fine needle set in a suitable handle. The point of the needle is dipped in turpentine, and when a shell is touched by it, it adheres and may be lifted out and deposited in a small vial, or upon the slide where it is to be mounted. I have a board about 6 by 10 inches, and 1 inch thick, in which rows of holes are bored nearly through. In these holes little half dram vials are placed. The vials are labelled with the names of the different species of Foraminifera, and when a shell is found it is picked up as described above, and with a little flip of the needle against the neck of the appropriate vial it is shaken off and preserved for future use. Sometimes I put all I find into one vial and then sort them out into their respective vials at my leisure. In place of the needle used as above, I sometimes employ a very small camel's hair brush, moistened and drawn to a delicate point by the lips. This is perhaps best for very delicate shells which might be broken by the needle. Of course, these manipulations will be difficult to the beginner till he learns how to use the needle under the objective, for he must remember every thing is reversed from what it appears.

The shells being obtained, how shall they be mounted? Very many of the species of Foraminifera, especially the younger shells, become so transparent when immersed in turpentine or balsam that they may be viewed by transmitted light. To mount them in balsam, the shells should be soaked in turpentine till all the air is driven out of the cells. Then they may be transferred to a slide with a cell deep enough to keep the cover glass from resting on them and which is filled with *thin* balsam. In this they can be arranged in position to display them properly. The slide should then be put away to permit the balsam to harden. Care should be taken to keep out the dust and that the balsam does not dry away from the shells and permit the exposed portions to fill with air. When the balsam has hardened, a drop of turpentine may be spread over the top and a fresh coating of balsam added. This may be repeated till the cell is full when with the last coat, or layer, of balsam the cover glass may be put on. This process may be greatly facilitated by the use of heat, but care is necessary to retain the shells in position, and such slides should be left lying flat.

Such mounts well repay the time and labour required in their preparation, for they not only serve for study with transmitted, but

with reflected light. Some species exhibit their structure beautifully when viewed as apaque objects when mounted thus.

The most ready, and the most common way to mount these objects is to place them dry in cells as apaque objects. For the purpose of study, or to make a systematic collection, it is best to mount several of one species only upon a slide, arranging them so as to show different stages of growth, difference of form, etc. To fasten them to the bottom of the cell, I first make a little dab of cement, using either stratena or liquid marine glue, upon the spot I design to place the shell with the point of a sharpened match. Care should be taken not to get on too much cement or it will penetrate the shell or form an unsightly mass surrounding it. Then I take up the shell with the forceps, or with the needle wetted by turpentine, as before described, and put it in place.

Not being satisfied with the ordinary slides and cells for this class of objects, I have devised a slide which serves the purpose admirably,—it is made as follows: The slide itself is of wood of the ordinary size and about $\frac{1}{10}$ inch thick. Through its centre is bored a hole ½ inch in diameter, over the back of this is pasted a strip of stout paper. The hole in the slide with the paper back constitutes the cell. In the bottom of the cell is pasted a disk of coloured paper, cut with a gun-wad punch, to serve as a back ground for the "mount." To give a neat finish a brass curtain-ring which just fits in the hole is fastened in with a bit of cement. The edges of the slide are now bound or covered, with a bit of coloured tissue paper. The shell may now be arranged in the cell, and the cover glass dropped in upon the brass ring, the top of which has been covered with cement. A suitable label, the whole size of the slide, is now pasted on the front, and a plain one may be put on the back.

Should a shell be very rare and it is desirable to show both sides, a piece of thin glass may be let into the back of the slide, and the curtain-ring placed upon this instead of the paper back-ground. Such a slide would need a hole in the back as well as in the front label.

When these slides are finished with pretty and suitable labels they make a fine appearance, pack and carry as easily as so many slips of wood, and if made of white bass wood do not warp. The porosity of the wood prevents any accumulation of moisture upon the cover glass, and upon the whole, these slides are very satisfactory.

I wish to call attention to the desirability of using some other colour for the back ground of apaque "mounts" than black. After much experimenting with various colours, I have settled upon that afforded by a pigment called "crimson lake." I use either this or a piece of glazed paper of the same colour. When the object is white, or nearly so, I use this colour exclusively, but when it has

some decided colour select that which seems to show it best. It may be a fancy, but I think any one who will take the trouble to experiment a little in this matter, will feel repaid by the improved appearance of the objects and the greater comfort in studying them. —*American Society of Microscopists.*

PREPARING AND MOUNTING BACTERIA.

By Prof. T. J. Burrill, Ph.D., F.R.M.S.

IN my work I have not attempted to compare the results of the various new staining materials, many of which I have not seen, but what follows is some account of ordinary laboratory manipulation with common analine colours, such as can be obtained at any drug store for the colouring agents. Want of sufficient chemical knowledge has made it impossible for me to prosecute any systematic experimentation for the accomplishment of new results, and the pressure of other duties has been too great for the record of such achievements as should be presented in a paper of this kind. I can, therefore, only beg the indulgence of my fellow-workers whose practice has been more considerable and whose attainments are much more commanding than my own. A full account of my attempts to properly stain bacteria would be the record of more failures than successes, judged from my own estimate of what the latter should be, and this, not only at the outset of experimentation on several species, but more or less throughout the work. It is true there are species that may be very readily stained with scarcely a failure, by the use of common aniline, violet ink and other dyes, and this, too, without after-washing, leaving the material in which the organisms are imbedded too deeply coloured ; but what seems altogether satisfactory for temporary examination in these cases does by no means answer for all purposes nor with all organisms. The stain is not effective, or not suitable, or not permanent, or defective in one or more of several other particulars.

I enumerate some elements of successful staining, as follows:

1. The organisms should be decidedly and conspicuously coloured. 2. The general mass of imbedding material should be left unstained, or so different in colour that the organisms can be distinctly seen. 3. There should be no granular or other precipitations from the staining material, nor should any portion of the latter remain as a coating on the glass. 4. The colour should be suitable for the purposes required, and permanent, if the object is to be mounted for the cabinet. 5. The process should be as simple as possible and free from manipulative difficulties.

No one staining material, nor any single method of procedure can be made to answer well these requirements for all kinds of bacteria, and in spite of successful practice upon many kinds, the operator must ever be prepared for failure in some one or more of the specified particulars. Still, the failures after a little experience may be accounted the exceptions and success the rule.

Except for a few special results, aniline dyes are by far the most serviceable in staining bacteria; but these dyes are very different in their effects, and require different treatment. Probably an aqueous solution of methyl violet, so commonly used as ink, has been more generally used for the detection and examination of bacteria than any other agent, though in the hands of some of the most skillful workers gentian violet and methylene blue have each been pronounced most highly serviceable for general purposes. Ordinary violet is, when effective, usually quick and intense, and there is from the aqueous solution no sedimentary deposits consequent upon evaporation or subsequent washing with water, and most often the colour of the mass can be suitably reduced by causing a stream of water to play from a wash bottle upon the material. There are, however, two objections to this stain, aside from the instances in which it entirely fails to colour the objects. It is not suited for photography, and it is apt to be very fleeting, especially if exposed to the light. For the first there is no remedy, but for the second something can be done. The stronger the dye or the longer its application, especially the former, the more permanent the colour, and sufficient washing may be practised to secure the required tint of the field. After, however, the use of the dye of any strength, the application of alcohol is sure to discharge the colour from everything; hence mounting in balsam is considered impossible. Treated with acetate of potash, the colour is more stable and holds good for some months, but gradually fades in most cases. I have recently used instead of the aqueous solution of this violet aniline a solution in glycerine, with, so far, very satisfactory results. This may not be new to others—probably is not—but was to myself when first tried, about two months ago. After my experience with the aqueous solution, I was surprised to witness the permanence of the stain under treatment with alcohol and of exposure to full sunshine for several days. The preparations are still in good condition, though nothing further can now be stated as to the length of time they will so remain. The aniline is soluble in glycerine to any extent, as is the case with most of the other aniline colours tried, and was used of a strength of about one to five in bulk. To this may be added a small quantity of pure carbolic acid without detriment, so far as observed, and with advantage in the way of effectiveness in some cases. There is no difficulty whatever in passing the stained material through alcohol and ben-

zole, or oil of cloves, into balsam ; but in the case of bacteria dried on the cover-glass these preliminary immersions are unnecessary. The mounting is direct in pure balsam, care being taken that the prepared material is thoroughly dry. The colour stands under subjection to the heat necessary for hardening the balsam. In the case of sections containing organisms, clearing agents are required, and may be freely used with at least certain species of bacteria—*e.g.*, those commonly found on the mucous surfaces of the human body. I have not noticed this difference in permanence of colour in the case of other anilines in glycerine ; neither do I esteem the solutions in the latter always better than those in water or in alcohol, but in all cases in which the glycerine solutions are as effective they are to be preferred to others on account of the ease of handling. The alcoholic dyes are much inclined to creep or run over or up the surfaces of the containing dishes, and he is an expert workman who can leave his practice with clean fingers, to say nothing of forceps, needles, and perhaps of the work-table. Besides this, the alcohol on exposure rapidly evaporates, and often precipitations and sedimentary deposits mark the results. I do not know how others avoid this escape of alcohol when either on account of the necessities of the stain or of other business it must be left many hours. I have nothing better than a nest of ordinary porcelain dishes, the edges of which when necessary are smeared with glycerine. Stoppered bottles are not convenient. For the aqueous solutions these dishes are all that can be desired, unless intense heat is to be applied, when watch glasses take their place ; but with the watery colours and much more so with the glycerine solutions, nothing is needed for a staining vessel. The dye is simply placed in a little pool on the prepared cover-glass, and the latter is held in pliers, or placed on a level surface, where, in the case of the glycerine, it may be left any length of time if protected from the dust. When ready it may be taken again in the pliers, washed by a stream from a wash-bottle with either water or alcohol, or both, without during the process losing account of the surface of the glass on which the material exists. If a second glycerine colour is to be applied, this can be done at once in the same way without stopping for drying or for other preliminaries.

Suppose one wants to examine the organisms in his own or any other person's mouth. He may proceed as follows : Secure a little mucus from the tongue or teeth, taking care to avoid the remnants of the last bread and butter enjoyed. Place the substance on a clean glass slip, and if necessary mix by stirring. Make a little spatula of wood a quarter of an inch wide and cut square at the end, with which smear a well-cleaned cover glass, after the manner of spreading blood by drawing the spatula with the material once or twice, side by side, over the desired surface, holding the instru-

ment at 30 or 40 degrees from the horizontal. Dry in the air or with moderate heat; then pass the glass through a flame somewhat quickly three or four times, holding the smeared surface up, and from a bottle, through the cork of which passes a glass rod, put on a drop of the glycerine violet; after one minute or less wash with water by means of a stream from a wash-bottle, and mount while wet for examination, the upper surface being wiped dry. If for permanent preservation, see that the cover glass and stained material is thoroughly dry; then mount directly in pure balsam, or, better in some respects, but not so good in others, in Farrant's solution of gum and glycerine. All this can be done without putting the object out of hand, and in a very short time, and our specimen remains fit for the cabinet or for re-examinations, months and probably years afterward. The use of an alcoholic solution of blue or red aniline would, with the same or similar treatment, be a complete failure, the whole surface, glass and all, remaining smeared with the dye in spite of water washing, and the whole colour disappearing with the use of alcohol. With other organisms, as, for instance, the micrococcus of pear blight, the aqueous solutions of red or violet aniline are partially successful, while methylene blue entirely fails, but the strong glycerine solutions of the two former give far better results.

A method of "fixing" aniline colours, devised, I believe, by H. A. Reevis, of England, has been tried by myself with successful results, though sufficient time has not elapsed to prove the durability of the stain. Alcohol does not remove it. The stained material, whether section of tissue or dried film on the cover glass, is first treated with a saturated aqueous solution of tannin and distilled water in equal parts, to which is added a little carbolic acid, for three to five minutes; then, after washing in water, submit to a strong aqueous solution of potassio-tartrate of antimony (tartar emetic) for an equal time, and after thorough washing in water transfer to alcohol, a clearing oil, and balsam. I have tried a number of processes for staining *Bacillus tuberculosis*, the now well-known organism of pulmonary consumption; but have succeeded only with the use of aniline oil and fuchsin in alcohol and water, as recommended by Ehrlich and others. In one instance only a good stain, apparently, resulted from this red aniline with aniline oil in glycerine; but I could not repeat the accomplishment, possibly, however, from the want of fresh material though with that tried the Ehrlich process succeeded. Dr. Gradle, of Chicago, succeeds with a saturated alcoholic solution of fuchsin in five to eight times the quantity of a 5 per cent. aqueous solution of carbolic acid. I have tried the same mixture without success, but my proportion of carbolic acid was considerably greater. Gibbes, of England, makes a distinction between fuchsin and magenta, but no such difference

is recognized by dealers in our country. A request for red aniline, or either of the above, is responded to by offering the same thing. Babes, of France, recommends a saturated aqueous solution, prepared with heat, of aniline violet 1 B. and an exposure of twenty-four hours, or the same with gentian violet. He further also modifies Ehrlich's method by making a saturated aqueous solution of aniline oil, using heat. To this is added 5 per cent. of absolute alcohol and 5 per cent. of a saturated alcoholic solution of fuchsin, or of a very concentrated solution of methyl violet 1 B. Twenty-four hours are again allowed for the stain, decolourizing with nitric acid and water, one part to four. I have prepared the dye used by myself as follows: Into twenty grammes of 85 per cent. alcohol, stir three grammes of aniline oil, and add this slowly while stirring to two grammes of pulverized crystals of fuchsin, the red aniline of the shops. To this solution add while constantly stirring twenty grammes of distilled water and bottle without filtering. This stain keeps well for some months, but gradually becomes unfit for use by the precipitation of granules of colouring matter. Doubtless this could be prevented by a little experimentation, thus making it possible to keep the dye in stock. In practice I have smeared a cover glass with a little sputum, dried and heated it as before described, and, having poured a little of the dye into one of a nest of porcelain dishes, have therein immersed the cover glass, perhaps several of them, put a little glycerine around the edge of the dish, and covered with another dish. This is left from two to twenty-four or even more hours when the cover glasses are taken out and freed from the adhering dye by washing in alcohol, usually by a stream from a wash-bottle, then immersed in nitric acid and water, one part to four, until all colour perceptible to the eye disappears, or usually about one minute. Wash thoroughly in water, and mount in water for examination. The special organisms should appear as minute ruby-coloured rods, in a white field. If desired, the latter is stained blue or green by these anilines in glycerine by one minute's application. If for a permanent preservation, after drying, mount by placing a minute drop of pure balsam on the centre of a slide, and invert the cover glass upon it. Harden with moderate heat. These organisms can be seen by sharp eyes without staining, and by becoming familiar with their appearances, one can pretty certainly pronounce upon their presence without any preparation, provided they are numerous in the specimen examined, but as they alone hold the stain by the above process, it is not only easier to detect them when coloured, but to more certainly know them from other kinds of bacteria very commonly found in sputa of the healthy, as well as the diseased.—*American Society of Microscopists.*

MICROMETRY.

AT the meeting at Chicago of the American Society of Microscopists last year, the "National Committee on Micrometry" presented a report of their proceedings, a reprint of which has now reached us. We learn from this report that at the Session in Indianapolis in 1878, the Society adopted a resolution referring to the various Microscopical Societies certain questions pertaining to Micrometry. Dr. R. H. Ward, who was then President of the Society and also of the Troy Scientific Association, addressed a communication to all the Societies interested, in consequence of which there was formed a Committee, which ultimately included Prof. W. Ashburner, of the San Francisco Microscopical Society; Prof. F. A. P. Barnard, American Metrological Society; Dr. Lester Curtis, Illinois Microscopical Society; Dr. G. E. Fell, Buffalo Microscopical Society; Dr. Henry Jameson, Indiana Microscopical Society; Prof. S. A. Lattimore, Rochester Academy of Sciences; Rev. S. Lockwood, New Jersey Microscopical Society; Prof. E. W. Morley, American Association for Advancement of Science; Dr. G. Richardson, American Postal Microscopical Society; Prof. W. A. Rogers, Harvard University; Prof. S. P. Sharpless, Boston Microscopical Society; Prof. H. L. Smith, Hobart College, Geneva, N. Y.; Prof. A. H. Tuttle, Microscopical Society, Columbus; C. M. Vorce, Cleveland Microscopical Society; and Dr. J. J. Woodword, U. S. Medical Museum, Washington.

For the unit in Micrometry, the Committee adopted the *micron* ($\mu = \frac{1}{1000}$ *mm.*), and for their standard a centimetre scale obtained for them by Prof. Hilgard, the Superintendent of the U. S. Coast Survey. This scale was verified "with great care" by Prof. C. S. Peirce in 1882, and again last year by Prof. W. A. Rogers. The scale was examined with a half-inch objective supplied with a Tolles' opaque illuminator, and the defining lines were found to be of the most beautiful character. The scale is marked on a plate made of platinum and iridium (10 $^0/_0$ iridium), and is divided into ten millimetres, one of the millimetres being divided into tenths, and one of the tenths into spaces of ten microns. The accuracy of the scale was determined by comparison with a standard which, it is stated, had been verified by Prof. Tresca at Paris, and Mr. Chaney, the warden of the standards at London. It was resolved by the committee that this scale, "Standard Micrometer, Centimetre A, 1882," should not pass out of the hands of the Treasurer of the Society, except to persons of eminent ability.

In connection with this subject, we may also refer to a paper recently written by Prof. W. A. Rogers on "A critical study of the

action of a diamond in ruling lines upon glass."—(American Society of Microscopists.) In this paper it is remarked that since the death of Nobert, Mr. Fasoldt, of Albany, stands easily first in the fine art of ruling. Prof. Rogers has recently taken up this subject with the view of testing the claim of Mr. Fasoldt, that he has succeeded in ruling lines one million to the inch, and especially by the claim that the existence of a spectrum in the bands is an evidence of the reality of the separate lines. We cannot learn that any one has yet succeeded in photographing a Fasoldt plate as high even as 100,000 to the inch. Photography, the author remarks, offers the evidence, somewhat negative in its character, that the limit of visibility, as distinguished from resolution, is reached with lines having a width of about $\frac{1}{200000}$ th of an inch, and lines of this width are the finest that have ever been photographed. An excellent example of Prof. Rogers' own gratings was presented this year to the Royal Astronomical Society, and one of his glass centimetres, containing 1001 lines to the centimetre, was presented to the Royal Microscopical Society. Those who are practically interested in diffraction rulings, will find much in Prof. Rogers' paper worthy of careful consideration; for it would almost appear that the microscope has, in this matter, reached its highest visual possibilities, little or no progress having been made (certainly none whatever in this country) since the resolution of Nobert's nineteenth band.

A BETTER KNOWLEDGE OF OPTICS.

THIS is perhaps the most important of all means or verification of microscopic observations. Without this all the rest will be in vain. The most elaborate or the simplest apparatus will yield no real gain of knowledge to the world unless the eye be trained to comprehend what it sees, to interpret the appearances that present themselves and discriminate the causes that produce them, and so trace back the effects of the lenses themselves, of the diaphragm, of the obliquity of the light, and the effects due to the real structure of the object under examination. The mathematic reasonings of Helmholtz and still more those of Abbe into the true theory of microscopic vision may not need to be followed by everyone who would use the instrument, but to be acquainted with the main facts of Abbe's theory—to comprehend the doctrines he has propounded and the experiments by which he has made it plain, so as to use it in the interpretation of what the lens reveals is as necessary for him who would be a well-skilled observer as for him who would improve the powers of the instrument itself.—*American Society of Microscopists.*

THE RELATION OF APERTURE AND POWER IN THE MICROSCOPE.

By Professor E. Abbe, Hon. F.R.M.S.

(Read 14th June, 1882).

II.—The Rational Balance of Aperture and Power.
(*ii.*) *Division of the Entire Process of the Microscope between Ocular and Objective.*

(Continued from page 192.)

(*b*) According to a fundamental dioptrical proposition the linear amplification N′ of the image, which is projected by a system of given focal length *f*, is *strictly* determined by the formula

$$N' = \frac{\Delta}{f},$$

in which Δ denotes the distance of the image from the posterior principal focus of the system (the place where rays are collected from distant points in front of the system); and this is the same whether the image be real or virtual. The objective-image of a given system is therefore always amplified in exact proportion to the length Δ; and the linear diameter of the dissipation-circles (ϵ) of that image must also be proportional to Δ, since ϵ is proportional to N′. Taking now the *angular* diameter of these dissipation-circles at the posterior principal focus, *i.e.*, the visual angle under which they would appear *at that place*, this angle must obviously be the same for every position and amplification of the image, because the linear diameter ϵ always varies in direct proportion to the distance Δ. We thus arrive at the theorem :—

If an objective projects a real or virtual image without the interference of an eye-piece, the visual angle of the dissipation-circles of that image, taken for the place of the posterior principal focus, is the same for every position and amplification of the image, and is a constant quantity in every system.

This proposition shows the method of estimating numerically the degree of optical perfection in objectives. The constant visual angle defined above (which I shall denote by the letter u in the following discussion) exhibits an exact measure of the smaller or greater dissipation of the rays *inherent* in a given construction, and one which is independent of the various accidental circumstances under which an objective performs.

(*c*) Suppose now the angle u (the inherent angular dissipation of the light) to be given for a certain objective, and an image projected by that objective to a distance Δ from its posterior prin-

cipal focus (which focus is generally in composite systems not very far from the back surface). The linear dissipation of the light in that image will be

$$\epsilon = \Delta\, u,$$

whilst the amplification of the object is

$$N' = \frac{\Delta}{f}.$$

This objective-image being observed by means of an ocular of a focal length ϕ, and a virtual image being projected to a distance l from the eye-point (the distance of distinct vision) the linear amplification N'' to which the objective-image is submitted will be

$$N'' = \frac{l}{\phi},$$

and the total amplification of the ultimate image

$$N = N'\, N'' = \frac{\Delta\, l}{f\, \phi},$$

which is the general and strict formula for the determination of the power of a compound microscope by means of the focal lengths of objective and ocular, and the distance Δ, which I shall call the optical length of the tube.*

At the same time we obtain the *linear* dissipation of the light at the ultimate (virtual) image, owing to the simple enlargement of the circles ϵ through the ocular,

$$E = N''\, \epsilon = \epsilon \frac{l}{\phi},$$

or

$$E = \frac{\Delta}{\phi}\, l\, u.$$

* As the focal length of a composite system is always the quotient of the linear amplification N of the image, by the distance of that image from the posterior principal focus of the sytem (which is in the case of the microscope the place of the Ramsden circle above the ocular, or the eye-point, very approximately), we have the *focal length of the entire microscope.*

$$F = \frac{l}{N} = \frac{f\, \phi}{\Delta},$$

where the length Δ may be defined now as the distance between the *posterior* principal focus of the objective and the *anterior* principal focus of the ocular, because this latter focus must coincide with the objective-image (very approximately at least) in order to obtain the ultimate virtual image at a considerable distance.

The *angular* diameter of the same dissipation-circles in the visual image is therefore

$$U = \frac{E}{l} = \frac{\Delta}{\phi} u,$$

which shows that the enlargement, by the action of an ocular, of the dissipation-circles which are inherent in a given objective, is numerically expressed by the quotient of the optical length of the microscope-tube by the focal length of the ocular.

If, for example, an objective of any kind be used with an ocular of say 1 inch, the length of the tube being such that the anterior principal focus of the ocular is 10 inches above the posterior principal focus of the objective, we shall have the *optical*-length of the tube $\Delta = 10$, $\phi = 1$, and the quotient will yield the number 10; and this will express the fact that under these conditions the dissipation of the light in the ultimate image of the entire microscope has a visual angle ten times as large as any image which is projected by the same objective without an eye-piece. This result, obviously, does not depend on the supposition of any definite distance of projection (l). The same will hold good for every position of the image, be it a virtual image (as in the ordinary use of the microscope) or a real one, as is the case when the image is projected by objective and ocular conjointly on a screen or photographic plate.

The foregoing proposition admits, however, of a simpler and more expressive enunciation still, which is shown by the above formula for the amplification of the entire microscope:

$$N = \frac{\Delta\, l}{f\, \phi},$$

which may be written:—

$$N = \frac{l}{f} \cdot \frac{\Delta}{\phi}.$$

In this equation the quotient $\frac{\Delta}{\phi}$ (which may be denoted by the letter ν) is one factor of the total amplification N; and the other factor $\frac{l}{f}$ indicates that amplification which the objective alone will yield for the same distance of projection (l). The value of $\frac{l}{f}$ which I shall denote by the sign [N] may be conveniently called the *normal amplification* (the *own proper* amplification) of the objective,

because it is realized when the objective is used without an eye-piece, as a "simple microscope."

We have now

$$N = [N] v$$

and conversely

$$v = \frac{N}{[N]}.$$

The value of v, which was defined above by the quotient $\frac{\Delta}{\phi}$, and which indicates the enlargement of the dissipation-circles by the ocular, is therefore also the quotient of the total amplification of the microscope by the normal amplification of the objective, and thus expresses the *increase of power*, beyond the normal power of the objective, which is obtained in the compound microscope by the tube and ocular combined. I shall, therefore, call the quantity v the *super-amplification* which is applied to a system, or which it has to bear when it is the objective of a compound microscope with a given length of the tube and a given ocular.

We arrive now at the proposition:—When an objective (for which the constant visual angle of the inherent dissipation of light is given) is used with any length of tube and with any power of the ocular, the angular dissipation is always increased in the ultimate image in proportion to the super-amplification which the objective has to bear, *i.e.*, according to the quotient of the total amplification of the microscope by the normal amplification of the objective.

The foregoing considerations lead to a comprehensive expression and measure of the combined effect of tube and ocular in the compound microscope, which holds good (as may be shown) in regard to *all* functions of the instrument. If, for example, we know that the objective of a microscope has a focal length $f = \frac{1}{2}$ inch—which gives the normal amplification $[N] = 20$, for a distance of vision $l = 10$ inches—and that this objective is used for a power of $N = 200$, we have a super-amplification $v = 10$. We have thereby analysed the composition of the total power of the instrument as between objective and ocular, or the manner of co-operation of these two elements of the composite system, in quite a general manner; and we know that all essential conditions of the optical performance remain the same as long as the same value of v is maintained, whatever may be the particular conditions as to length of tube and depth of ocular. At the same time we have established a numerical test of the *strain* to which an objective must be submitted in order to obtain a certain total power of the microscope. We know that if an amplification of 200 is required with a 1-inch instead of a 1-2 inch as in the preceding example,

the necessary super-amplification will be $=20$, and all aberrations and other defects inherent to the system will appear in the image under twice the visual angle of that in the other case.

(d) In order to compare the performance of *different* objectives under various powers, it will be necessary and sufficient, according to the foregoing theorems, to determine for any given system the constant quantity u, by which the inherent dissipation of the rays is measured.

One part of this problem may be settled by means of the following proposition :—With objectives of equal aperture, similar construction, and equal degrees of technical excellence, the constant visual angle of the dissipation-circles is always the same *and independent of the focal length.*

This may be proved by a very simple consideration. Suppose a system A of a certain aperture and given focal length f to be brought to the best possible correction of which the construction may admit for a certain distance of the image. Another system B of exactly similar composition may now be obtained by reducing the linear measures of *all* the elements (all radii, diameters, distances, &c.) and all technical defects of figure and positions of the lenses in the same proportion (say, *e.g.*, of $2:1$), just as if the diagram of the system and the transmitted rays had been drawn on a reduced scale. The focal length will thereby be changed in the same proportion ($f : \frac{1}{2}f$) and also the distances of the conjugate foci of best correction; but the aperture will not be changed, and the angles of all emerging rays—of regular or irregular transmission—will be the same as the angles of the corresponding rays in A, by virtue of the strict geometrical similarity of all the elements. If now the space over which the rays of *one* pencil are dissipated at the image of A, subtends a certain angle u in regard to the posterior principal focus of A, the same angle u must obtain for the image of B in regard to the corresponding principal focus of B; and that angle u must persist, as has been shown, if B should afterwards project an image to any other distance (*e.g.*, at a corresponding distance to A), provided the best correction for the new position of the conjugate foci be obtained. Consequently the angular value of the dissipation-circles *(u)* is the same for all *similar* systems, however different the focal lengths may be.*

* Strict similarity cannot of course obtain, except when the distances of the conjugate foci, for which the objective is corrected, are proportional to the focal length. We may, however, disregard all differences of construction which could be affected, or undone, without introducing new essential aberrations; and those changes of a system which are necessary in order to compensate for different positions of the image, belong to that kind. The proposition will therefore hold good for all objectives of a *similar formula* and equal aperture.

The assumption of a proportionate reduction of the geometrical defects (defects of figure, centering, &c.) with decreasing focal length, which is implied

Though this proposition allows a comparison of those objectives only which have equal apertures and are of similar construction, it leads to important inferences. Firstly, it shows that the characteristic constant quantity u, which is the "measure of perfection" of the objectives, does not require a separate determination for every single system. If the value of u be known for one system, it is known for all systems *of the same kind*, *i.e.*, for all which have the same aperture, are constructed on a similar formula and with an equal degree of technical skill. Secondly, the proposition indicates the method by which a direct comparison of objectives of *different focal lengths* may be obtained in regard to the *quality* of images of equal amplification.

Suppose the angular dissipation of the light—the constant angle u—to be given for a particular kind of objectives of definite aperture. If any one of these objectives has a focal length $=f$, its normal amplification is $[N] = \frac{l}{f}$ ($l = 250$ mm. or 10 inches). If now the total amplification of the microscope is required $= N$, the necessary super-amplification to which the said system must be subjected will be

$$v = \frac{N}{[N]} = \frac{N}{l} f,$$

and this super-amplification will introduce an angular dissipation of the rays at the ultimate images, which is shown by the expression

$$U = v\,u = \frac{N}{l} f\,u.$$

Consequently: For objectives of the same kind and the same aperture, but with different focal lengths, the manifestation of the inherent defects and aberrations under a given power of the microscope is always in direct proportion to the focal length (or in the inverse proportion of the objective-power) by which such amplification is obtained.

2. If we could suppose objectives of ideal perfection—absolutely free from all technical defects and all unavoidable aberrations—the

in the demonstration above, will not be in full accordance with the actual circumstances. In practice the *relative* accomplishment of smaller lenses will be inferior to that of larger ones. According to the experience of the author, the difference is, however, not very considerable except when the dimensions are very minute. Though some difficulties of exact workmanship are increased with smaller dimensions, there are others which are diminished; and the relative amount of those defects which cannot be overcome by careful and skilful work, may therefore be considered as nearly equal for all focal lengths, within rather wide limits.

quantity u would be = 0. In that case the dissipation of the light at the ultimate image of the microscope would also = 0, *i.e.*, the image would retain its full ideal perfection, with every amount of super-amplification; and it would, therefore, be entirely unimportant whether a certain total power had been obtained by an objective of long or short focal length. If, however, the objectives in question are afflicted with certain defects, however small, the quantity u will obtain a certain value; and the dissipation of the rays corresponding to that value, being more and more enlarged as the super-amplification is increased, there must always exist a certain maximum super-amplification or value of ν which the objective will bear without a visible or an objectionable loss of sharpness or perfection of image. Consequently, we have a maximum of the total amplification N which can be obtained with a given focal length under the condition of a certain degree of sharpness of the image; and, conversely, a maximum focal length which admits of a given amplification under the same conditions. If, for example, the inherent dissipation of a certain kind of objective were confined to an angle of 15″, no eye would recognise the dissipation-circles if such a system were used only under its own normal amplification (using it as a simple microscope). The dissipation would, however, become visible, and would introduce a perceptible indistinctness of the image, if the super-amplification much exceeded 4, and the deterioration would become very great should it amount to 16, because in these cases the circles of indistinctness would be displayed in the respective ultimate images under a visual angle of more than 1′ and of 4′. If now a certain amplification, say 320 diameters, is required with objectives of that degree of perfection, a 1-8th inch would yield that number with a super-amplification of 4, but a 1-2 inch would require 16; the perfection of the image in the latter case being very much less than in the former.

It would be useless to attempt to assign by way of example numerical values of the constant u for different kinds of objectives, and of the limit of U which would be consistent with a sufficient perfection of the image, in order to compute theoretically the amount of super-amplification which every objective would bear. The circumstances on which the first two elements depend, are much too complicated for a theoretical estimation of their influence in regard to the actual performance of the microscope. Nevertheless, the foregoing considerations indicate the aim of the problem, which is to determine the adequate focal length for every aperture. It will be quite sufficient for our purpose to determine the limits of admissible super-amplification *directly* by practical observations, without further caring for the elementary conditions on which it depends; and if this is done, we have obtained all necessary data for the problem under consideration.

Having already settled the total amplification N, which is required for the utilization of a given aperture,* we need only to find the super-amplification ν which an objective of that aperture will bear, without a perceptible depreciation in the quality of the image if objectives up to the present standard of excellence are supposed. The quotient $\frac{N}{\nu}$ will then indicate at once the normal amplification [N] of the objective which is necessary in order to obtain the said N under the best possible conditions; and having thus determined [N], the quotient $\frac{l}{[N]}$ will yield the focal length which an objective of the aperture in question ought to have for utilizing the delineating-power of that aperture in the most favourable manner. (The focal length thus assigned for a given aperture will be expressed by millimetres or by inches, according as l is taken = 250 mm. or = 10 inches.)

Though the problem in this way leads us to practical questions which are to be answered by observation, apart from all theory, it will not be useless to point out some theoretical considerations which may elucidate certain experimental facts, or guide the observer in experiments on that subject.

(*a*) One fact which we may foresee in theory, is that the limit of useful super-amplification must depend on the *aperture* of the objectives, and that the former must diminish with increase in the latter. The greater the aperture the wider the range for the deviation of the rays from the ideal collection of the pencils to mathematical image-points. All technical faults of the lenses—slight defects of figure and of centering—must give rise to increased deviations, and therefore to an increased amount of their accumulated effects, because the clear diameter of the lenses which transmit the pencils bears a greater ratio to those radii of curvature which are required for the wider aperture. Exactly the same holds good with the strictly residuary aberrations which are the predominant source of defective performance in modern objectives (the unavoidable technical faults being much less apparent with the excellence of workmanship which has now been attained).—*J. R. M. S.*

(To be continued.)

* See this Journal, ii. (1882) p. 463.

BACTERIA IN BRICKS.

REFERRING to the alleged discovery by M. Parize of bacteria not only on the surface but in the interior of bricks, the *Lancet* call the attention of the public to the fact that bacteria are not necessarily all hurtful. "We are not told," says the *Lancet*, "precisely what sort of micro-organisms were discovered, but it must be remembered that even bacteria are susceptible of classification into useful, harmless, and noxious. There is no good reason for being alarmed at the presence of these germs in burnt clay, for the atmosphere itself is known at times, probably always, to contain varying numbers of them. It is certainly not wise to conjure up new sanitary dangers on the receipt of information not altogether of a novel character. Let the public, therefore, not be alarmed." We may add that what may be spoken of as the legitimate function of bacteria is still an open question. It is, for instance, a question how far they are useful in aiding the processes of digestion, and Dr. Angus Smith has even asked whether water entirely deprived of living organisms can be considered good or wholesome water in relation to the animal economy. The presence of living germs in burnt bricks is, however, apparently another curious illustration of the varied conditions under which bacteria can live; and even if harmless bacteria are to be found in bricks, it is not unlikely that deadly ones might be found there also. Meanwhile the alleged discovery may well attract the notice of M. Bechamp, who will probably be disposed to contend that the brick bacteria have been developed by the heating process from the geological microzymes in the original materials.

OUR BOOK-SHELF.

COLE'S STUDIES.—The different series of Mr. Cole's work continue to be issued weekly. They comprise Popular Microscopical Studies; Studies in Microscopical Science; Section I., Animal Histology; Section II., Vegetable Histology, and the Methods of Microscopical research, which last will form an introductory essay to Volume II. When we consider how well Mr. Cole has done his work it is surprising he has not met with more support, and we can only come to the conclusion that the demand for *educational* slides and for *real instruction* in connection with the microscope is very limited indeed.

The first volume of these "Studies" forms an extremely interesting and instructive book, and the second volume (so far as it has gone) is in no way inferior to the first. We earnestly hope that every Provincial Society has added at least one copy of these "Studies" to its library.

THE JOURNAL OF THE POSTAL MICROSCOPICAL SOCIETY.—This Journal, which at the commencement of its third volume added "The Journal of Microscopy" to its title, continues to be exceedingly well conducted, and we hope it has the hearty pecuniary support of all working microscopists. The July number contains the following articles:—Some new Infusoria from Bristol; the Collection and Preservation of Diatomaceæ; some further researches on Tubifex; the Action of Ammonium Molybdate on the Tissues of Plants; the Microscope in Palæontology; Diamonds and their History; the Study of the Larval forms of the Crustaceæ; Hydrozoa and Medusæ, an examination of the External Air of Washington; on the Peronosporeæ; Selected Notes from the Society's Note Books, &c., &c.

On page 194, a correspondent, signing himself "A President," makes the following statement:—"*In plain sober truth there is at present no recognised organ in which the various papers and other matters of interest can be recorded.*" We are surprised the Editor did not call attention to *The Microscopical News* which *is* the recognised organ of the Liverpool, Manchester, Bolton, North of England, and other Microscopical Societies. Other Societies commenced well in sending their proceedings, then came abstracts, then short notes, and finally nothing. There are more difficulties in the way of publishing a record of several Microscopical Societies than "A President" seems to be aware of; experience must teach these, and we hope that if the "Journal of the Postal Microscopical Society" undertakes this task it will be more successful than we have been. At the end of the present year we may perhaps say something more on this subject.

The American Monthly Microscopical Journal publishes a list of the preparations circulated in the American Postal Club, with notes thereon: could not this be done in the English Society?

NOTES AND QUERIES.

As we wish to relieve ourselves of all purely business transactions in connection with the Journal, subscribers are kindly requested to pay the amount of their subscriptions to Messrs. Brook & Chrystal, 11, Market-street, Manchester.

ALL matter intended for publication must be sent before the 12th of each month to the Editor, Mr. George E. Davis, **Belmont, Thorncliffe, nr. Sheffield.**

BLACK VARNISH.—There was a black varnish which seems to have

been used by some of the older workers in microscopy, and was not affected by the usual solvents, alcohol, benzole, acetic acid, or potash. Can any of your readers inform me of what ingredients it was composed ?—*Vau.*

Manual of the Infusoria.—The Publishers of Saville Kent's work contemplate issuing an edition with coloured plates if twenty subscribers are found. This is an excellent opportunity for Local Microscopical Societies, if they have not already purchased the work. Orders may be sent to Mr. Thomas Bolton, 57, Newhall Street, Birmingham.

Septic and Pathogenic Organisms.—The Medical Department of the Local Government Board have just issued an interim report by Dr. E. Klein, M.D., F.R.S., "On the relations of Septic to Pathogenic Organisms,"—a *brochure* of 40 pages of exceedingly interesting matter. It may be obtained for five stamps from Messrs. Knight & Co., 90, Fleet Street, London.

Mounting Fresh Water Algæ.—A correspondent well versed in the mounting of desmids and other similar vegetable productions writes us as follows :—After trying all sorts of media with glycerine, carbolic acid, camphor, chloride of calcium, &c., in their composition for mounting fresh water algæ, I came to the conclusion that none was so good as plain water, with the least addition of camphor water to prevent fungoid growths from being afterwards developed.

Most media have a specific gravity greater than that of water, and their effect is always to drive the endochrome into the middle of, or at any rate away from the walls of the cells, whereas when plain water is used its natural disposition remains long unchanged. But I am sorry to say that I have never succeeded with *any* medium in preserving the green colour in any fresh water Algæ with any degree of certainty. I have *some* slides of Desmids mounted ten years ago, *and still as green as grass*, and others mounted last year, which are already brown, and I cannot tell why this is so. It does not seem to depend on the season at which the plants are collected. I am not sure whether two precautions might not lead to success in this point, and I will make experiments shortly on the matter. I would suggest, 1st, to use water recently boiled, and then closed up in a flask so as to minimise the amount of air dissolved in it; and 2nd, immediately after mounting to put the slide in the dark. I have before now had good specimens spoiled by the plants going on growing after mounting, and eliminating oxygen from the carbolic acid in solution in the water. As I imagine the brown colour to be due to oxidation of the endochrome these precautions might be of some use. A. W. W.

BECK'S "COMPLETE" MICROSCOPE LAMP.—The base consists of a heavy ring, into which a square brass rod is screwed. The square rod carries a socket with an arm, to which the lamp is attached. This socket fits the square rod loosely, but is kept in any position by a lever which is pressed firmly against the square rod by a strong spring. If the lever and the opposite side of the socket are taken between the thumb and finger, the pressure of the lever on the bar is removed, and the lamp can be raised or lowered to the desired position, when by releasing the hold the lamp is at once clamped.

On each side of the burner, and attached to the arm, is an upright rod, to one of which the chimney is fixed, independent of the reservoir of the lamp, but fitting closely over the burner, thus enabling the observer to revolve the burner and reservoir, and obtain either a thin intense light or a broad and diffused one, without altering the position of the chimney. The chimney is made of thin brass, with two openings opposite to each other, into which slide 3×1 glass slips of either white, blue, or opal glass, the latter serving as a reflector.

The reservoir, although holding enough oil to burn for several hours, is made very flat, and drops into the annular base, thereby bringing the flame of the lamp within 3 inches of the table, rendering it much more serviceable for direct illumination (without the mirror) and for other purposes.

A semicircle swings from the two uprights, to which it is attached by the pins, placed level with the middle of the flame; to this semicircle is fixed a dovetailed bar, carrying a sliding fitting, which bears a Herschel condenser. This condenser, swinging with the middle of the flame as a centre, is always at the same distance from it; and thus, when once focused, needs no further alteration for any change in the inclination of the beam of light. The condenser is fixed at any inclination by a milled head working in a slotted piece of brass, fixed to the arm.

When used for transparent illumination, the condenser is not required below the horizontal position; but when the lamp is required for the illumination of opaque objects, the chimney having been temporarily removed and the milled head fixing the condenser arm having been loosened, the arm with the condenser can be thrown over the lamp, as shown in the illustration, and the chimney being replaced, the light, which now comes through the opposite opening of the chimney, can be condensed at a large angle below the horizontal.

NEW IMBEDDING MATERIAL.—Celloidine is an article made by the Schering Chemical works of Berlin. It makes a perfectly transparent imbedding medium, a collodion absolutely free from

sediment; noted for the fineness of its film, and for its non-contractile property.

OBJECTIVE CHANGERS.—A correspondent writes asking us which of the various "changers" we recommend, or whether we still have preference for the old "screw."

We have tried the various patterns as they have been brought out, and to this date have determined to stick to the "old screw," as although we are often hard pressed for time we have never found the so-called instantaneous changers to enable more work to be done, and we have even discarded the double nose-piece in ordinary work.

NE QUID NIMIS.—Ray tells us that "Enough is good as a feast," but the editor of the American Microscopical Journal seems to regard a modicum as sufficient from the following paragraph extracted from page 111 of the June number of the journal. "The great fault with most books on mounting seems to be that the instructions are given too much in detail." We cannot agree to this statement. We have never seen a single book (not even "Practical Microscopy!") in which the details have been given as fully as we should like to see them described.

THAMES MUD.—An exceedingly valuable paper by Dr. Lionel Beale, F.R.S., on "The Constituents of Thames Mud," may be found in the February (1884) number of the Journal of the Royal Microscopical Society. This paper should be studied by all chemists who may be interested in water analysis.

PROCEEDINGS OF PROVINCIAL SOCIETIES.—We wish it to be understood that our pages have never been closed to the Transactions of Local Microscopical Societies. *The Northern Microscopist* was first started to keep a record of the doings of Northern Societies, but after a time it was found necessary to reduce the list of objects exhibited at each meeting on account of the many complaints from our readers. We have always given every facility for the publication of papers read before Provincial Societies; most of our contributors have been supplied with free copies, and the cost of illustrations has, in no case, fallen upon the authors. Perhaps some of our readers will be good enough to disseminate this information.

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

No. 45. SEPTEMBER. 1884.

WEEVILS.

By James Fleming.

A paper read before the Manchester Microscopical Society.

WHILE it is generally understood that all kind of agricultural and garden produce suffer from the attack of insects, the extent of the damage and the serious loss involved thereby is only known to those who have to do with the cultivation and "handling" of the products of the soil. Although these pests make their presence known to the florist and farmer by the stunted growth and sickly appearance of the plant, and by the perforations and reduced bulk of the grain, they are frequently so small that the naked eye can hardly discern them, and it requires the microscope to reveal their true character.

If I enumerate a few instances of loss occasioned by lice, flies, and beetles, they may help to justify my venturing to bring under the notice of the members that most destructive family of coleopterous insect—Weevils.

In August of last year there appeared an article in the *Northern Microscopist*, giving an account of the devastation in the vineries of a gentleman at Accrington, from an attack of the vine-aphis, *Phylloxera vastatrix*, the same insect which causes such havoc in the vineyards of France. In this instance the ordinary skill of the gardeners on the spot failed to discover the cause of all the mischief, and only by using the microscope was the enemy discovered.

As a constant guest, but in varying numbers, the British farmer has to contend with the wire-worm, or click-beetle, belonging to the family Sternoxi. The larva takes its name from having the appearance of a piece of flattened wire, while the jumping motion of the imago, producing a clicking sound, furnishes its name. As in most instances, this beetle does the greatest harm when in the larval condition. In this state it continues for sometimes as long as five years, eating away and attacking the roots of all crops,

excepting the mustard plant, and such is their tenacity of life they will withstand the application of quicklime. The extent of the loss to the farmer is often equal to half the value of the entire crop.

Perhaps there is hardly a worse pest to the agricultural interest in this country than the larva of the crane-fly, or daddy-long-legs, *Tipula oleracea.* [Two years ago we had an almost total failure of the hop harvest, resulting from an unusually severe attack of the hop aphis. As a consequence, hops went up from £4 to £40 per cwt., and some brewers were known to use chemicals, quassia chips, and other abominations, as substitutes for hops. The loss to the planters was estimated at the sum of £1,750,000, say one million and three quarters sterling, and the loss to the field labourers amounting to £200,000.]

There is an insect called the wheat-midge, *Cecidomyia tritice*, and its grub is known as the red maggot. "This insect is of a minute and delicate form; it deposits its eggs amongst the glomes of the flowerets of the wheat by means of its ovipositor, which is very long and retractile within the body." These gnats measure $\frac{1}{8}$ of an inch in the body, and have very long legs and horns in proportion; they are immensely prolific, as many as thirty-five have been counted on a single ear of wheat in the act of ovipositing. The larva are hatched in eight or ten days; they eat the pollen, and thus prevent the formation of the corn. The loss sustained is incalculable.

Another species of this family ravages the American wheat. It attacks the plant as soon as it appears above ground, and entirely prevents its growth by eating into the stem. It there goes by the name of the Hessian-fly and the scientific name *Cecidomyia destructor.* The alarm occasioned in the British mind some years ago caused the Privy Council to pass a stringent law to prevent its importation. Much more recently, however, the British mind was exercised in reference to the approach of the dreaded Colorado beetle, which had committed such havoc abroad.

Considering the magnitude of the depredations of insects all over the world, but more especially in hot countries, it is surprising that so little importance is attached to the subject, book writers usually dismissing it in a few lines.

My connection with the rice and corn trade brings under my observation the extensive damage done by Weevils on foreign importations, and it is easy to see that, whatever precautions are taken, it is impossible to prevent foreign genera being imported, and the exotic becoming a perfectly naturalized animal.

All the rice, and more than half the wheat consumed in this country is imported, and nearly every cargo that arrives is infested by Weevils. It is difficult to ascertain the loss; we have no exact

data, but an average cargo say of 2,000 tons wheat may be depreciated to the extent of 2s. per cental, equal to about £4,000.

Some idea of the devouring propensity of Weevils may be gathered from a statement a corn merchant made to the writer. This gentleman, when residing in New South Wales, went into an Iron Corn Store, which was filled with sacks of corn. On inquiring what the noise was he heard he was told it was caused by Weevils boring and eating the grain. He compared the noise to a loud humming, or like machinery at work. He could hardly hear his own voice.

The wheat-weevil *Calandra granarius* is referred to by Professor Westwood as an insect of minute size, not exceeding ⅛th inch in length, but which, from attacking stored up corn, frequently commits incalculable mischief. The female deposits her eggs in each grain, the mealy interior of which is entirely consumed by the larva. Professor Duncan says, "The female deposits her eggs on the corn when it is stored up, and the young grubs burrow into the wheat as soon as they are hatched, each individual occupying a single grain. They there undergo their metamorphoses, and then come out perfect beetles, and lay their eggs for a fresh brood. Another species called the rice weevil, *Calandra oryzæ*, finds a habitat in that grain. The difference between the two species are so slight that the one is taken for the other, and their mode of propagation is the same.

In the proceedings of the Entomological Society there is a letter addressed to a F. L. S. on this subject. This gentleman differs with the opinion that *Calandra granarius* perforates the grain, and there deposits her eggs. He is certain from personal observation that the animal lays her eggs in the blossom of Indian corn, and that the corn is formed with the egg in the heart. He examined it with a microscope, and found no signs of perforation anywhere, although the chrysalis was evidently there. He further states he could hatch them at 110° Fahr., but that 130° would kill them.

With regard to the latter statement I am informed that kiln drying, which is usually a much higher temperature than any he mentions, does not always kill them, and that the only effective way is steaming at 70lbs. pressure, which is equal to about 315° Fahr. Cold, however, they cannot stand; hence it happens that very few weevils are alive when shipped in the winter months. For the rest of the statement it is no doubt true that female weevils deposit their eggs in the blossom of the plant, and in this habitat they pass through their metamorphoses. If the season be cold this process is retarded, the imago period is reached, and the insect escapes before the outer skin hardens; but in a hot season the epidermis quickly hardens and imprisons the creature. And it is also quite true that breeding goes on after the grain is

garnered by boring and ovipositing therein, so that the statements are not conflicting, for both are right.

Packard remarks that the weevil family may at once be recognised by the head being lengthened into a long snout or proboscis, used for boring into objects when about to oviposit, near the middle of which are situated the long, slender, elbowed antennæ. At the extremity of the snout are situated the mouth parts, which are much reduced in size, the palpi having small rounded joints. Their bodies are hard, and generally round, and often very minute. They are very timid, and quickly feigned death. The larva are white, thick, fleshy footless grubs, with fleshy tubercles instead of legs, and are armed with thick curved jaws. They feed on nuts, seeds, the roots, pith, and bark of plants, leaves or flowers, and especially the fruits, while some are leaf miners, and others are said to make galls. Preparatory to transformation they spin silken cocoons. The number of species already known is immense, being not less than 8,000 to 10,000, and upwards of 630 genera have been already described by Schönherr and others. The genus Calandra has a slender snout slightly bent downwards, a coarsely punctured thorax, nearly half as long as the whole body, while the elytra are furrowed, and do not quite cover the tip of the abdomen.

The grain Weevil, *Calandra sitophilus*, the one under consideration, is pitchy red in colour, and immensely prolific; the surface rough. It is about ⅛th inch long. This great pest, both as larva and beetle, consumes wheat after it is stored up, being very abundant in granaries. The larva devours the inside of the hull, leaving the shell whole, so that its presence is not easily detected.

Sitophilus oryzæ attacks the grains of rice and also of wheat. It differs in having two large red spots on each elytron, and it is abundant in the south, where it is called the "black weevil."

As we get the bulk of our grain from America, Packard's statement, as an American naturalist, is important. So far as the description goes he concurs with English entomologists, but there is a difference in classification, and in the generic and specific names, making identification, in the first instance, to amateurs a little confusing.

That the diet of these creatures is selective there is doubt, but failing one dish they avail themselves of another, and so we find by personal observation that the weevil *Calandra granarius* passes from wheat to rice, and *vice versâ*.

After his usually exhaustive way Westwood further remarks, "As may be expected from the great extent of this family the modification of structure amongst the exotic genera are almost endless. The form of the body in some is quite linear and attenuated, in others globose or oval; while the surface is in some

smooth and polished, in others completely covered with tubercles, and in others squaremose. The legs, again, in some are disproportionally long, especially the anterior pair, and in others the prosternum is armed with one or two long porrected spines." I beg to call your attention to another insect of the Beetle order known by the trade as a Weevil, but inasmuch as it does not possess the notable characteristics—the long snout and elbowed antennæ—it cannot rank as a true Weevil. It is as destructive and as plentiful on imported wheat and rice as *C. granarius.* There is a question about its scientific name. *Pediacus dermestoides* has been suggested by a competent authority, but the figuring of that insect by Spry and Curtis do not support that opinion. *This* beetle does not appear to be figured in any books—the dentated lateral margin of the thorax makes it easy to be detected, and, although so common, this important variety in structure appears to have passed unobserved. The nearest figure and description appears in Westwood's Introduction (Vol. 1, p. 149), under the order *Necrophaga*, family *Engidæ*, and it is probably the genus *Cucujides piceus*, or *C. testaceus;* both of these insects are grain-feeders, and are found in granaries. On the other hand, the genus *Dermestidæ* usually infest the carcasses and skins of dead animals, and from which, indeed, they derive their specific name. The one under notice is about the eighth of an inch in length, brilliantly red in colour, and the elytra present a shining appearance. Mr. F. Enoch says he has ceased to mount these insects as they are unsatisfactory. I must invite your inspection of some I have mounted in balsam, and I think you will say they are satisfactory; not only is the structure defined under the microscope, but they are pleasant to look at, especially so when shown with the spot lens.

The sketch before you represents the Bean "Weevil" *Bruchus rufimanus* (red handed). This animal is found in large quantities in the large flat beans imported from foreign parts. Belonging to the order Rhyn chophora, the sub-family is distinguished by the antennæ being filiform, or but slightly thickened at the tips, serrated or pectinated, the eyes imaginate; the rostrum broad and deflexed; the elytra do not cover the abdomen, and the hind legs are often very large. Scientifically speaking *B. rufimanus* is no weevil, as it does not possess the elongated proboscis and the elbowed antennæ required to place it in the class Calandra, while it is more than twice its size. The munching of grain is a common practice of corn dealers, no doubt a capital thing for dentists, but a questionable dainty, for lurking concealed in the corn lies Bruchus, and the muncher soon finds out, by a pungent bitterness of flavour that the delicate morsel is a mixture of vegetable and animal food.

Concerning it Westwood, in his matchless way, remarks, "The

perfect insects are found upon plants, appearing during the period of flowering, and depositing their eggs in the small and yet tender seeds of the luguminous plants, as well as in various kinds of corn, palms, &c. In these seeds the larva finds not only a secure habitation, but also a plentiful supply of food; and in which it subsequently undergoes its transformation until its arrival at the perfect state, when it makes its escape by gnawing a small round hole through the rind of the seed, the larva having previously eaten its way to the inner surface of the seed, so that a thin pellicle alone remains; through which the larva makes a circular incision, having only a very thin pellicle in that part, through which the imago easily forces itself. One of these insects, *Bruchus pisi*, causes much injury to the edible pea by eating the interior of the seed, and making its escape when the peas are just ready for gathering. This insect, which is probably an imported species in this country, occasionally abounds to such an extent in some parts of North America, as to cause the total destruction of the crops of peas, &c.

"*Bruchus granarius* is also in this country often very destructive to the same vegetable, sometimes depositing an egg in every pea in a pod. In general, the insect remains in the larval state until the following spring, but if the weather be very warm, the perfect insect appears in the preceding autumn; the larva has the curious instinct to leave the most vital parts of the seed until the last. The larva (Germar) is a soft white and fleshy grub, with a scaly head, and stout strong jaws, with the legs obsolete or but slightly developed. They have nine spiracles on each side of the body." Of this family there are upwards of 300 species, the body of *rufimanus* is covered with black downy hairs mottled with white, the legs and antennæ are of a bright orange colour, and the head is folded underneath the breast.

Usually the perforations in the grain indicate that the creature has bored its way out; but when not suspected of being therein, there being no outward signs, on splitting open the bean, the insect may be found coiled up either dead or alive; and if the weather is very warm at the time (if alive when released from bondage), it has been noticed to stretch its untried pinions and fly away. Is there any other creature, except perhaps the frog, which can live apparently without air or light for so long a time?

Notwithstanding the efforts of millers to eliminate all foreign matter from wheat by the employment of the most modern machinery, their ingenuity cannot entirely prevent Weevils being ground up in the flour.

In the transactions of the Entomological Society I find the opinion of a medical man to be that he considers the wings and crustaceous part of insects so heating to the system as to be almost

as injurious as *Cantharides* taken internally on a slow scale,—referring to the Spanish-fly, *Cantharis vesicatoria*, or blister-beetle, so well-known in commerce and imported from Spain for medical purposes. On the other hand, the Rev. W. Hope, F.R.S., refers to the Levitical law (Lev. xi. 21, 22), where permission is given for the people to eat "beetles, locusts, and grasshoppers." Mention is made of an Indian king treating his guests with the larva of an insect instead of fruit, which, it is stated, would probably be a species of Calandra, which is widely spread over the Asiatic continent.

Calandra palmerum, an insect two inches long, is said to be roasted by the natives of the West Indies, and is esteemed, when properly cooked, rich and delicate eating. Linnæus confirms the delicacy of the fare. The same Rev. gentleman regards the Baptist's food in the wilderness to be the locust migatoria, and not the pods of a species of cassia. He also gives a list of eighteen species of beetles which are eaten in hot countries, and observes, "Considering that all the insects alluded to live on clean vegetable diet, for the most part, consequently afford more wholesome food than some of the animals usually served on our tables," he could see no reason why they should not be eaten.

Dr. Moffatt, the African Missionary, declares locusts to be good food, and almost as good as shrimps.

In these days of food adulteration one gets uncommonly suspicious.

In finely dressed flour there is very little chance, if any, of contamination from beetle matter, that is supposing beetle matter to be contaminatious; but in wheat-meal, or undressed flour there is greater liability, and if the wheat is infested, and no imported corn is free, notwithstanding the efforts of millers to remove all impurities, you will be sure to get more or less of weevil matter in it. By a consensus of opinion, oat meal is regarded as the most nutritious of all food for English people, and, singular to say, there are no weevils or any devouring pests to be found in oats, that is, in the matured grain. Query, Is this the reason for their extra wholesomeness? In the presence of medical men one feels treading on dangerous ground, and I only venture to name the matter with a view to elicit information. I have not seen the subject mooted, and perhaps it is a little irrelevant to the subject of my paper.

There is a wide field, almost unexplored, opened to microscopists in the examination of articles of food; up to the present it has been in the hands of professional gentlemen, and of these only a very few are workers. The way to work is to get a pure sample and compare suspected samples with it, *i.e.*, in this case, get a quantity of wheat free from weevils, have it ground and

dressed, and let this be the standard for comparison. The real difficulty in such microscopical investigation is in getting a pure standard sample; but with this absolutely secured, all difficulties vanish, and every microscopist should be able to detect adulteration easily and quickly.

THE RELATION OF APERTURE AND POWER IN THE MICROSCOPE.

By Professor E. Abbe, Hon. F.R.M.S.

(Read 14th June, 1882).

II.—The Rational Balance of Aperture and Power.

(*ii.*) *Division of the Entire Process of the Microscope between Ocular and Objective.*

(Continued from page 213.)

THE sources of the residuary aberrations in question are perfectly well known in theory. Some of them result from the disproportionate increase of the positive and negative spherical aberrations in different parts of the system, arising from the increase of obliquity and in regard to different colours, which disproportionality prevents a strict compensation of the opposite spherical aberrations even for the rays of one colour and gives rise to still more considerable residuals in the totality of the rays of mixed light.

Other defects arise from the disproportionate increase of the dispersion from the red to the blue, which is found in all kinds of optical glass hitherto produced, and forbids a really perfect chromatic correction of the systems. In regard to all of these aberrations it may be readily shown that they *must* introduce greater and greater uncorrected residuals as the numerical aperture of the cone of collected rays is more and more increased, other circumstances being equal.*

It is therefore obvious that under equal conditions of technical construction the inherent dissipation of the light will always be greater with the wider apertures, and consequently the super-amplification which is compatible with any given degree of precision of the image will be confined to a *lower* figure with

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* That the numerical aperture is the essential element and *not* the aperture-angle, results from the fact that all the effects considered above depend on the proportion of the clear aperture to the focal length of the objective, which proportion is exactly expressed by the numerical aperture.

objectives of wide than with objectives of low aperture, which inference is fully justified by experience.*

(*b*) On the other hand, theory indicates different conditions for the residuary aberrations, with even the same (numerical) apertures, when objectives of *different systems—dry and immersion*—are compared. The uncorrectable residuals of the aberrations will always be greater when the total amount of aberrations requiring correction is greater. Now the front-aberration, which is a very predominant part of the total amount in dry lenses of somewhat wide aperture, is considerably diminished with water-immersion and almost entirely suppressed by the homogeneous-immersion system. We expect therefore a higher value of admissible super-amplification in the case of homogeneous immersion than in that of water-immersion; and a still higher for water-immersion than for dry—provided always that objectives of the same (numerical) aperture are compared; and conversely, one and the same super-amplification will admit of an equal degree of perfection of image with a greater aperture in objectives for homogeneous-immersion than in water-immersion or dry lenses—which is also in accordance with the facts.

(*c*) Another point which deserves particular attention in every attempt to assign the proper relation of aperture to power, relates to the great influence of the *illumination* and the *nature of the object* on the visibility of the residual defects of the objectives. If we could determine numerically the inherent angular dissipation of the rays (the angle u) for a given objective, either by computation or by experiment, the value of u would then indicate the visual angle of the circles of indistinctness in an image which is obtained under the normal amplification of the objective (if, for instance, the objective were used without an eye-piece, as a hand-magnifier) and $U = v\,u$ the same visual angle for a super-amplification of v—

* The above statement does not of course imply the opinion, that an objective of lower aperture should, under *all* circumstances, admit of a higher super-amplification practically than one of wider aperture. The "definition" of a lens, in the generally adopted sense, is quite another thing to the dioptrical precision of the image, which is in question here. There may be lack of "delineating power" when a certain amplification is reached, and then every increase of the amplification renders the impression of the image worse and worse, notwithstanding the utmost perfection of the dioptrical performance of the lens. If, for instance, an objective of 0·1 N.A. were made with the short focal length of a 1-8th, it would not bear even the lowest eye-piece, because the normal power of the system (80 diameters) is already an empty power for so narrow an aperture, whilst a well-made 1-8th of 0·8 N.A. will give a satisfactory image with a relatively strong ocular. If, however, an objective of 0·1 N.A. has a focal length of say 1 inch, it will bear a deeper eye-piece than a 1-8th of 0·8 without any perceptible loss of sharpness. In order to compare microscopic images in regard to their dioptrical conditions, the strange element of "lack of definition of empty powers" must be excluded.

but both elements under the obvious supposition, that the *whole* pencil of light which is collected by the objective, is under comparison, or the full area of the aperture effective at the same time. If in any particular case a portion only of the clear aperture should be utilized by the delineating pencils, the actual dissipation of the light will of course be confined to more or less small spots than would correspond to the angles u and U, and would accordingly become less apparent. In the practical use of the Microscope we always have very variable conditions, according to the illumination in use and the structure of the objects under observation. With very low apertures the range of difference is not so great, it is true, because the illuminating cone of light will generally fill the whole aperture, or at least the greater part of it. But with wide-angled objectives the incident beam from the illuminating apparatus is—and in most cases must be—confined to a much narrower angle than the aperture of the system. How much of the aperture is actually utilized by the delineating pencils will entirely depend on the dissipation of the incident rays by the structure of the object—in particular the diffraction effect of the structure; and according as the illuminating cone, after its transmission through the object, is spread out to a smaller or greater angular extension, smaller or greater aberration-circles will disturb the image. Thus it may happen that with one kind of preparation an objective may bear a deep ocular very well, whilst with other objects a great deterioration of the image becomes visible under even a lower ocular. Objects which show a regular striation, and in general all regular periodic structures, are particularly *insensible* to the residuary defects of the objectives, because they produce only a limited number of *isolated* diffraction-beams, and thus leave the greatest part of the objective's aperture entirely unemployed. In observing an object with only one set of parallel lines which are near the limit of the resolving power of the objective, only two small portions of the aperture are simultaneously utilized, one by the direct beam, the other at the opposite edge of the opening by the diffracted beam, as may be ascertained by a glance at the objective's clear aperture. All defects and aberrations of the system which inhere in the inactive portions, do not exist for the image in that case, whilst they will at once become effective when those objects of a very complicated and irregular structure, which produce a continuous and widely spread out diffraction-pencil, are observed. This consideration will show that the ordinary test-objects of the Microscope—particularly lined objects, and in a somewhat less degree all kinds of diatom markings—are the most unsuitable preparations for a proper judgment of the performance of the instrument in regard to the *general* conditions of scientific work inasmuch as the latter are always much less favourable than those of diatom observations.

3. In the face of the many intricate circumstances hinted at in the foregoing discussions, a *numerical* estimation of the super-amplification which is favourable or even admissible with various kinds of objectives, must be a very difficult if not impossible task. It would rarely be possible to assign any measure which would receive the general assent of microscopists, because so many elements are concerned in the question which cannot be estimated apart from the individual opinions of the observers.

One particular difficulty in observations for this purpose is to decide whether a given objective is really up to date or is afflicted with accidental defects, which might be avoided by more accomplished workmanship, and must therefore be disregarded. Another drawback to an accurate estimation arises from the before-mentioned very different sensibility of the image to difference of structure; and not the least of all is the large amount of personal equation which is always met with, when a judgment as to difference of quality has to be formed in regard to microscopic images. Moreover, every one who estimates the value of the element in question must be conscious of its provisional character. For whilst the amount of super-amplification which a system is able to bear with a certain degree of perfection of the image is the true standard of the progress of microscopical optics, the determination of the said element cannot claim anything more than a temporary value: those figures of ν which may very well conform to the present conditions will not be true for the microscopes of a former period, and will perhaps be upset within a few years by the further progress of optical science and art.

I have now made observations for the purpose in question during many years—studying the performance of a large number of objectives of various kinds and various origins, upon very different objects (artificial preparations and natural objects), and checking my own observations by the judgments of some experienced working microscopists in the department of Biology. What I consider as the outcome of that systematic trial will be briefly indicated here, with all that reserve which is necessitated by the nature of the problem. The principal points are:—

(1) With the best *wide-aperture* objectives which have been made anywhere up to the present time (1882), dry or water-immersion, of apertures not less than 0·80 and 1.10 respectively, the deterioration of the image by the manifestation of aberration-effects becomes visible as soon as the super-amplification (ν) in use is greater than about 4 times;* that is to say, that any trained observer would

* I leave out of consideration here some particular objectives (recorded in this Journal, vol. ii. (1879) p. 815) which were made some years ago by C. Zeiss, for experimental purposes, on a system of construction which is not applicable to, and was not intended for, regular use.

recognize a decided falling off in sharpness and definition in the images in comparing two objectives of equal aperture under the same total power, when that power is obtained with one objective (of shorter focal length) by a fourfold super-amplification, and with the other (of longer focal length) by a perceptibly higher one, *e.g.*, sixfold; and that the *advantage will always be found on the part of the lower ocular-power*—whilst *no* advantage will be gained when the same power is obtained with a still more diminished value of v (less than 4). It being of course always understood that objectives of equal and best attainable construction are compared on sensitive objects, and that only the central portion of the field of vision is considered.

For example, if a total amplification of 480 diameters is obtained in one case with an objective of 1-12th in. focal length, and in another case with an equally good 1-8th of the same aperture—the figures of v being now 4 and 6 respectively—my view is that no practical microscopist would hesitate to declare the image of the 1-12th to be the *better* image; provided suitable preparations (of complicated structure) be observed; though *not* probably on simply lined objects and perhaps not on diatom-markings of any kind. On the other hand, no decided advantage of any kind will be recognized if, instead of the 1-12th, a 1-18th or 1-24th of equal aperture is used for obtaining the same power of 480 diameters, with of course lower ocular powers.

Hence it appears that the inherent dissipation of the rays arising from technical defects and residuary aberrations remains, in carefully finished wide-angled lenses of the dry and the water immersion-system, below the threshold of distinct vision as long as it is not enlarged by more than four times, but it is elevated beyond that threshold with every greater enlargement. That my observations do not indicate a decided difference between dry and water-immersion lenses may be well accounted for by the fact that the advantage of diminished front-aberrations in the immersion system is balanced by the increased aperture. With immersion-lenses of not more than 1·0 or less (other circumstances being equal), a somewhat higher value of v would be found. On the other hand, I have always observed a perceptible lowering of the *critical* super-amplification with objectives of greater apertures than 0·90 for the dry system, and of 1·20 for the water-immersion, when preparations are used for the experiment which put the utmost marginal zone of the aperture in action *simultaneously* with the intermediate portions between the centre and the margin.

(2) A decided advance in the performance, in regard to the critical value of v, is found in well-made objectives of the homogeneous-immersion system. With the same standard of judgment, and on the same principle which has been explained above, I con-

sider a super-amplification of about 6 as that which will just raise the inherent aberrations up to the threshold of vision, for an aperture of about 1·30.* It appears quite intelligible that the total (or nearly total) suppression of the front-aberrations should not only compensate for the increased aperture but in fact should leave a surplus benefit, as is indicated in the higher value of ν.

(3) Regarding the lower apertures of the dry system, my comparisons show a relatively *slow* increase of the critical ν, as the apertures diminish. This may be sufficiently accounted for by the circumstance that these lower apertures are always made with relatively greater working-distance (the *clear* air-space between the front surface and the radiant being a greater fraction of the focal length) than is adopted in the wide-aperture systems.

The relief to the front-aberration, and the corresponding reduction of the residuary aberrations, which is due to the reduction of the angle of the pencil, is therefore partly compensated by the increased aberration attendant upon a relatively thicker air-space in front of the system. Considering the medium-power objectives as they are generally (and properly) made with the view to a convenient working distance, I cannot admit of a higher number for the critical ν than 5 to 6, even for apertures down to about 0·40 (47°). If the aperture is reduced below this, the increase of ν becomes decidedly more rapid, in so far that for 0·15–0·20 N.A. (17°–23°), 8 to 10 appears to me to be the correct super-amplification which very good objectives will bear without a perceptible loss of definition (under the condition, of course, that the total powers obtained thereby are not empty powers in regard to the delineating capacity of the aperture in question).

Similar indications for still lower apertures would be of very subordinate interest; and, besides that, they could not be given on

* If any one should wonder at the *low* super-amplifications assigned here, and should consider the above statements to be poor evidence of the present condition of microscopical optics, I would ask him to reflect upon what it means, that objectives even of rather short focal length should bear a super-amplification up to 4 and 6, without any perceptible injury to the sharpness of the image. This means nothing less than that the Microscope is capable of showing objects enlarged to more than 800 diameters under the same conditions, so far as the geometrical precision of the observation is concerned, as if the microscopic objects could be enlarged in that degree *corporeally*, not optically, and were then seen with the naked eye at a distance of 250 mm., without the interference of any optical apparatus. Up to those high figures of amplification the modern Microscope maintains therefore the undiminished sharpness of naked-eye vision, and performs without any perceptible difference in the same way as if material bodies, instead of mere enlarged images, were depicted upon the retina. The time is not long past when no system, except very low-angled lenses, could bear even its own proper power without any super-amplification, and not 100 diameters could then be obtained without great inferiority when compared with direct vision.

the basis of reasoning established here, which depends on the condition of a *constant* visual angle of the inherent dissipation of the rays for different distances of the objective-image. This condition (as has been observed) holds good with sufficient approximation only as long as that distance—the length of the microscopic-tube practically—is not *too* small a multiple of the focal length of the objective; this is *not* fulfilled, under ordinary circumstances, with the very low-power systems which would come in question for apertures of only a few degrees.

4. The values of v assigned above for different kinds of objectives express, in my opinion, the conditions of the *best possible* performance of the microscope under present circumstances. I by no means contend, however, that much higher super-amplifications might not still be very useful; but if it cannot be denied that with the objectives which are made at this date, a *better* image is obtained under a four-fold, or six-fold, super-amplification than can be obtained under a higher figure, it is absolutely certain that the lower powers ought to be used, when the *utmost attainable degree of perfection* is required. It is quite immaterial for that conclusion, whether the loss of sharpness ("definition") attendant upon higher values of v, may be deemed small or great, and whether it may become obvious with all preparations or with a few only. If there *is* a loss, however small, and if only *one* kind of object is found with which it can be perceived, this alone will be sufficient to prove the advantage of the lower numbers; for there cannot be a reasonable doubt, that even the slightest difference in the perfection of the microscopic image may become a matter of decisive importance in critical cases of difficult research.

(To be continued.)

BACTERIA OF THE CATTLE DISTEMPER.*

THE bacterium of the cattle-distemper has been hitherto known almost exclusively in the bacillus condition, not making its appearance in the blood till some ten hours before the death of the animal. F. Roloff has examined the blood in the early stages of the disease, and also those organs, especially the spleen and the lymphatic glands, in which the bacilli are first seen. In all these he found a large number of small round shining bodies of micrococci. The infection of other animals with blood containing these cocci, produced in them the ordinary distemper with its bacilli, showing that the two are stages of development of the same organism.—*J. R. M. S.*

* Arch. Wiss. u. Prakt. Thierheilkunde, ix. (1883). See Bot. Centralbl., xvii. (1884) p. 112.

NOTES ON SOME FREE-SWIMMING ROTIFERS.

By J. E. Lord.

IT was not my intention to send any further "Notes" on the above subject, but since my last article appeared I have been receiving communications from many unknown correspondents, in several cases accompanied by tubes of Rotifers, upon which my opinion was desired. Mr. T. Bolton, F.R.M.S., the well-known microscopists' provider has been particularly kind in this respect. Two or three free-swimming forms, which have thus been brought before my notice, I have thought worthy of being included in a fourth and last communication, seeing the amount of interest which many naturalists are just now exhibiting in these animals. One very peculiar form, of which Mr. Bolton sent me drawing and description only, is of some interest on account of being a new creeping form. In this particular it recalls the two genera Ichthydium and Chætonotus, which were, on very slender grounds, included by Ehrenberg among the Rotifera, but which, from the absence of a rotatorian organization, were degraded to the Infusoria by Dujardin. From the drawing it appears to be somewhat fusiform, with the back convex, and the under-surface concave; ciliated anteriorly; tail-foot forked; it has also four prominent tactile organs, one pair on the neck, and another pair on the posterior part of the back. When mature, it is covered with a gelatinous sheath, through which the terminal setæ of the tactile organs just protrude. Unfortunately, Mr. Bolton's figure does not clearly indicate the internal organs, and it does not show any mastax at all; however, as Dr. Hudson has named it *Notommata spicata*, he must have been satisfied upon these two points. From the same source I received a large, handsome Brachionus, which Mr. Bolton believes to be *B. amphiceros.* If it be that species it is much larger than "Pritchard" makes it, and it has a remarkable peculiarity not mentioned by that authority. His description, too, is very meagre, it being dismissed in seventeen words. The peculiarity to which I refer is, that the four long, posterior spines are not rigid extensions of the lorica, like the anterior ones, but are so attached as to be movable, and the Rotifer can cross the points of all four of them, while freely swimming, and the lorica too is so thin that the animal can bend down the sides at the posterior (where it is normally very broad) until they are parallel. If these points are confirmed by other observers, they will form two good specific characters, as I am not aware of any other Brachionus having these two peculiarities.

In Fig. 26, I have drawn an interesting Rotifer which Mr. Bolton

sent me, along with drawings and description. It is very transparent; truncated anteriorly; cylindrical; arched dorsally; deeply plicate ventrally; cilia in bundles, moved by many powerful muscles (which are well shown if the water is allowed to dry up on the glass slip); eyes two, red; two short anterior spines; one

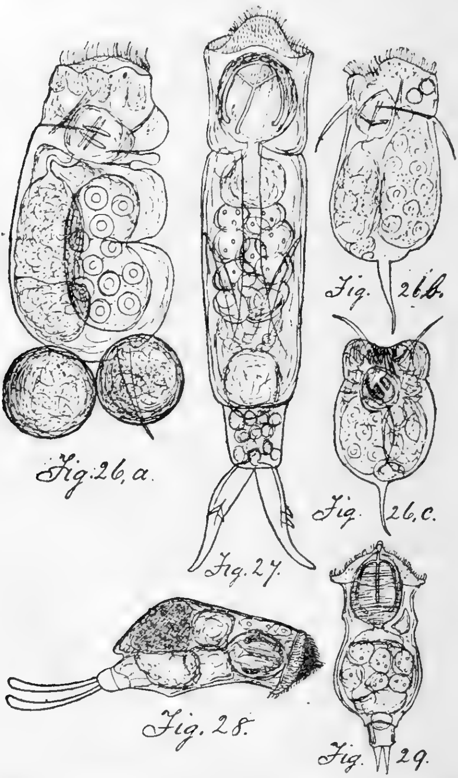

Fig. 26, a. Fig. 26 b. Fig. 26 c. Fig. 27. Fig. 28. Fig. 29.

posterior; jaws, presenting many different appearances, according to the position of the animal (some of which are shown in sketches); æsophageal tube, long and curved; ovary large, in some cases dark and segmented; many specimens were seen carrying two round eggs, as in Fig. *26a*.

Both Mr. Bolton and myself were at first inclined to consider it as *Monocerca bicornis*, although he had an inkling of the truth, but was misled by thinking it had *three* anterior spines. On pointing out the slight error into which he had fallen he then became convinced that his second supposition was the correct one, and, as on a more

extended acquaintance with it, I also came to the same conclusion, we named it *Triarthra breviseta* (Gosse). *Monocerca* has a styliform foot, but in this animal the appendage has none of the characters of a foot. Then, too, the numerous muscles, and long curved æsophageal tube point to the genus *Triarthra*. The discoverer's description is very short, and I am not aware that any microscopist has since come across the animal. It may be watched for a considerable time without showing the least trace of the anterior spines, and then all at once it may retract the ciliary region, when the spines, which are about one-fourth the length of the animal, will be thrown up. Fig. 26*a*, large specimen swimming; *b*, smaller one beginning to retract its cilia, showing anterior spines just leaving its sides; *c*, another small one, with cilia completely retracted, and spines erected. Fig. 27, *Furcularia forficula*. Characters, cylindrico-conical; obtusely pointed in front; foot, short, cylindrical; toes, long, stout, recurved and dentate (not at the base, but about the middle); one eye, frontal; cilia covering anterior pointed region; internal organs, as figured. According to Perty this Rotifer differs from *Distemma forficula* only in its single frontal eye, *Distemma* having two, cervical. My specimens did not agree in several points with the description of either the two animals named. The point, however, to which I wish to draw particular attention is the following, viz., its possession of two *internal* spines. The animal suddenly draws in its anterior part, and the two internal spines are thrown out laterally, the points pressing out the integument quite away from the internal organs. I have never satisfactorily made out the point of attachment of these spines, but several circumstances seem to indicate that they arise dorsally, and not far from the base of the foot. Several specimens I had on a glass slip frequently went through the performance mentioned, so that I never had any difficulty in seeing the spines. Mr. Bolton, however, writes me to say that he has not been able to satisfy himself about them. No Rotifer, so far as I am aware, has ever been described with such a peculiarity, and I shall be extremely pleased if other microscopists can confirm my observations. It would also be interesting to know if *Distemma forficula* is possessed of these spines. Fig. 28 is another Rotifer of the same genus as the last. I merely figure it to show that in all my specimens the toes get broader towards the free end. I have also come across another Rotifer similar to the last, but smaller, and with longer toes, which is characterised by the same peculiarity. As this is so different to the stereotyped toe I shall be glad to hear if others have noticed such a departure from the usual type. Fig. 29 is a charming Rotifer found upon decaying leaflets of *Myriophyllum* at the bottom of one of my aquaria. I am not aware of having seen it before, although I am a Microscopist of about 15

years standing. It is one of the quickest swimmers among the Rotifera. It swims quickly round in circles of varying dimensions, and then varies its movements by turning head over heels. It is a regular little fidget, and it was only after a considerable amount of manœuvering that I was able to get it sufficiently quiet to take its portrait. Perhaps the most remarkable point about this midget of a Rotifer is its immense gizzard! I was utterly at a loss as to its identity. I was at first inclined to consider it as belonging to the genus *Syncheta*, both from its rapid movements and its two anterior lateral lobes, although I was unable to make out any setæ. However Dr. Hudson informs me that he has little doubt of its being *Notommata lacinulata*, a very imperfect description of which will be found in "Pritchard." Your readers will also be glad to hear from the same source that Dr. Hudson's monograph of the Rotifera is now nearly completed, and that before long we may expect an announcement of particulars.

REMARKS ON PATHOGENIC BACTERIA.

By Dr. H. J. Detmers, F.R.M.S.

Mr. President and members of the American Society of Microscopists:—It was my intention to prepare a paper to be read before the Society at this meeting, but circumstances beyond my control prevented its preparation, consequently when coming here, I came to learn and to listen, and not to speak. But as you, Mr. President, desire that I shall say something, although entirely unprepared, the members of the Society, I hope, will be forbearing, and be satisfied with such a rambling talk, as I shall be able to give. It was my intention to write on "The Actions of Pathogenic Bacteria" (the name *bacteria* used as a generic term), because in my investigations of infectious diseases of live stock, I could not help observing that pathogenic bacteria do not all act alike. So, for instance, while many, perhaps most of the known pathogenic bacteria—particularly *Bacillus anthracis*—act as a ferment, cause fermentation or decomposition of the blood, others, for instance the micrococci, or more correctly, diplococci of swine-plague, do not produce any chemical changes in the blood and other tissues, except such, as will necessarily result from the withdrawal of such matter, as may be appropriated by them for their existence, and for their development and propagation. Otherwise they only act in a mechanical way; they close the finer capillaries, thus cause local congestion and embolism, and in that way produce the morbid process,—their action, at any rate, cannot be called a

specific one. That such is the case, I have proved as early as 1878 and 1879, as will be seen by reading my reports to the Commissioner of Agriculture. Nevertheless, M. Pasteur as late as last year, claims the discovery of the diplococci of swine plague as his. But so it goes, certain Frenchmen, and M. Pasteur, although otherwise a great man, is one of them, are ever ready to appropriate any convenient piece of literary or scientific property that is not closely guarded, or not kept under lock and key.

In my investigations I made another observation, that is, the same pathogenic bacteria may, under certain conditions and surroundings, be very malignant, and be producing disease in every instance, while under others they appear to be far less malignant, or even harmless.

An abundance of proof that such is the case can be furnished. In the first place, it is a well-known fact that almost all epidemic and epizootic diseases, but particularly such as are unquestionably caused by bacteritic growths, are more malignant at one time, or in one year, than at other times or in other years, and cause more losses in one epidemy or epizooty as the case may be, than in another, that many persons or animals respectively, though much exposed to infections, remain exempt, or do not take the disease, although there can be hardly any doubt that while exposed, their organisms have taken up a larger or smaller number of pathogenic bacteria. Such—that in every epidemic some persons, and in every epizootic some animals, remained exempt—at least was the case in regard to all those infectious diseases that came under my observation, and I am sure, any practising physician will testify to the same fact. Proof, however, would be more definite, if the blood, etc., of persons or animals thus exposed, but not affected by disease, had been oftener subjected to a microscopic examination.

Some time since Drs. Brown and Curtis, of this city (Chicago), attended a boy affected with tetanus. The boy had been fooling with the toy-pistol. Dr. Curtis examined the boy's blood (taken from a puncture in the finger) and found numerous micrococci. The doctors thought they had made a discovery, and searched for the probable source of those micrococci. Near the house they found a stagnant pool of water, and Dr. Curtis, putting some of the water under the microscope, found in that water, apparently, the same micrococci, at least, as to size and form, as in the boy's blood. The doctors, however, did not stop there in their investigation. Dr. Curtis further examined the blood of the boy's mother and sister, to all appearances perfectly healthy persons, and to his astonishment again found the same micrococci in considerable numbers. Then Dr. Brown concluded the micrococci could not possibly be the cause of tetanus, for, he argued, if they were,

the boy's mother and sister must have the same disease. Dr. Curtis did not express an opinion. Of course, I will not, and do not say that micrococci do constitute the cause of tetanus, and all I do say is that the presence of the same micrococci in the blood of two apparently healthy persons, the boy's mother and sister, is no proof whatever that those micrococci, seen in the blood of the boy, did not constitute the cause of the disease. The boy recovered, otherwise Dr. Curtis, in carrying on his research with his usual care and precaution, might have made an important discovery.

If the following facts are taken into consideration, it will not appear to be impossible that the micrococci, after all, bear a causal connection to the disease (tetanus). In the first place, a micrococcus is abundantly small to pass everywhere with the greatest ease where a blood-corpuscle can pass. Secondly, in many infectious diseases, looked upon as being caused by micrococci, the latter may be found in the blood circulating in unaffected parts of the body, but always as micrococci, seldom as diplococci, and *never* as *micrococcus chains* and *zoogloea-masses*, while in the affected tissues the latter forms are abundant. This shows that the formation of zoogloea-masses and chains has something to do with the morbid process. According to my observations, the latter, in swine-plague at least, is the result of embolism, caused by zoogloea-masses becoming lodged in, and thus obstructing, the finer capillaries. As long as a person or an animal is healthy, or as long the blood is regularly circulating in all parts of the body, no zoogloea-masses, it seems, are formed, and no obstructions occur caused by micrococci, but as soon as the current of blood in some part or parts of the body becomes irregular or retarded, perhaps through a sudden contraction of some capillaries caused by whatever agency it may be—a short exposure to a draft of cold air appears to be sufficient—the conditions, it seems, are given for the formation of zoogloea-masses, and all further consequences follow. Returning to the tetanus case, it is a well-known fact that toy-pistol wounds, and in short all those wounds frequently followed by tetanus, are, as a rule, lacerated, attended with more or less congestion, and, being exceedingly painful, show irritation of the nervous system. The congestion or irregularities in the capillary circulation, it seems, is most pronounced in the capillary system of the irritated nerves. The rest is easily explained, particularly if it is taken into consideration that tetanus in some localities is a frequent, and in others a very rare or entirely unknown disease. If statistics were taken, it would soon be possible to map out the tetanus districts, and it would probably be shown that all those localities in which tetanus is of frequent occurrence, have one thing in common, namely, a high stand of the surface water.

Another illustration is afforded by the Southern, Spanish, or so-called Texas cattle fever, which, undoubtedly, is a bacteritic disease. The pathogenic principle (the bacteria) which seems to develop on the grasses of the South, and is probably taken up by the cattle with drinking stagnant water, or by eating semi-decomposed grasses, does not visibly affect the native cattle, while, on the other hand, cattle imported from the North will take sick, and almost invariably (at least in a majority of cases) will die within a short time, often within a few weeks. But the native southern cattle, if taken from their native range and driven North at the end of the winter, after the spring rains and warmer weather have set in, or, in other words, after the old grass of last year's growth has commenced to decompose, and after new, young grass has made its appearance, will spread death and destruction wherever they go, while they themselves will remain healthy, or at least do not become affected with southern cattle fever. These southern cattle seem to carry and to propagate the pathogenic bacteria of the southern fever in their saliva. At any rate, if northern cattle graze where the grass has been contaminated with the slaver or saliva of such southern cattle, or if they (the northern cattle) drink water from ponds or water-holes, etc., which have been polluted with the slaver of southern cattle, the disease (southern cattle fever) is sure to make its appearance, and to become very destructive among the northern cattle after a period of incubation varying from three to thirteen weeks. But these northern cattle, even if suffering from the fever, are not able to infect other cattle that may come in contact with them, occupy the same pastures, or frequent the same watering places. To put it in a nutshell: cattle that take the disease do not communicate it, either directly or indirectly, and cattle that communicate it do not take it. Further, as already stated, southern cattle, as long as they occupy their native range, possess immunity. But this immunity disappears if the southern cattle are taken a certain distance further south, for instance from Northern Texas or the Indian Territory to Southern Texas, or from Southern Texas to Cuba, or other parts of the West Indies; again, if southern cattle are taken North, and are kept there one winter, because in that case their susceptibility or predisposition will become just the same as that of native northern cattle. These clinical facts, not only observed by myself, but also by a great many others, go far to show that the bacteria of southern cattle fever require, in order to become malignant (pathogenic) certain conditions, which are not existing, or perhaps have become exhausted in the southern cattle while on their native range, but are again reproduced if these same cattle are wintered, even if only one winter, further north, for instance, north of the Indian Territory. Also, that further south, for instance, in Cuba, much

less of these conditions is required than in Southern Texas, and in Southern Texas much less than in Northern Texas or in the Indian Territory. What these conditions consist in is not, or at least, but partially, known. Difference in climate and temperature seems to be of some influence, but of hardly enough to afford an explanation. Some classes of bacteria, as is well known, after a certain length of time, or after having reached a certain stage of development, exhaust (sterilize) their medium, or the subject they live in, and all propagation ceases till they are transposed to a new, fresh medium. Some species even make a further propagation in the same medium impossible by their own products. So it may happen that in certain infectious diseases, the first attack produces more or less immunity against further attacks. In most bacteritic diseases, however, in swine-plague for instance, this immunity is only partial, and not lasting. I have observed that one and the same hog—notwithstanding that a hog under domestication is for obvious reasons only a short-lived animal—suffered three times from swine-plague, and cases in which an animal was attacked a second time are quite numerous. Such cases undoubtedly would be still more frequent if the disease (the first attack) were not so fatal as it generally is. In regard to southern or Texas cattle fever, too, the immunity enjoyed by southern cattle, while on their native range, is invariably destroyed if these cattle are taken north, and kept there over winter. Consequently, M. Pasteur's inoculation theories need not to be tried on so-called Texas cattle fever.

(*To be continued.*)

OUR BOOK-SHELF.

The Chemistry of Foods, by James Bell, Ph.D., Principal of the Somerset House Laboratory. Pt. I., 120 pp. Tea, Coffee, Cocoa, Sugar, &c. Pt. II., Milk, Butter, Cheese, Cereal foods, &c. London: Chapman and Hall.

The South Kensington Museum has done much good in its time by the dissemination of useful information of various kinds, but the publication of a series of Science Handbooks is one very good means of educating those living at a distance from the great Metropolis, which has not generally been attempted before.

Mr. James Bell, in his preface to this work, tells us that it "is intended partly as an aid to those who may desire to carefully examine the illustrations of Food adulteration in the Bethnal Green branch of the South Kensington Museum, and partly as a contri-

bution to the already published knowledge of the composition and analysis of articles of food," and we are bound to say that the work has been excellently done.

Under the head of Tea, the configuration of the leaf is clearly shown, the epidermis of the true tea leaf, and an illustration is given also of ground tea. The microscopist should study these food stuffs in a state of purity, for then it would be comparatively easy to say whether any foreign ingredient was present or not.

Some time ago we endeavoured to get a well-known mounter of microscopic objects to prepare a series of slides of these food-stuffs for sale, as we feel sure there would be a call for them amongst food analysts, but so far we have not been successful in our efforts. If a series were prepared, it should be on the lines laid down in Notes and Queries of our next number.

Mr. Bell tells us minutely how tea should be examined, and gives us micrographic illustrations of the leaves of the elder, the willow and the sloe, some time since very extensively used in tea adulteration.

Passing to Coffee, we have an illustration of the true berry, of chicory, Mangold Wurzel, turnips, bean, locust-bean, acorn, fig, and date stone, and we must compliment the author on the excellent character of the engravings.

In the article upon Cocoa, an illustration is given of the genuine article, and illustrations also of the various starches used to mix with the so-called "soluble cocoas."

Honey has been somewhat neglected, and we would suggest that more be written in a subsequent edition. Microscopically, honey is very interesting, and this may be a hint for readers of papers before our Provincial Societies.

The microscopical analysis of milk and butter has been much worked at in America, but evidently Mr. Bell does not consider the results which have been published of much importance; in fact, he alludes to the subject in the following words:—"At one time fat prepared for making spurious butter was almost invariably found to possess a crystalline structure, and whether in a separate form or when mixed with genuine butter there was no difficulty in detecting the crystals by the microscope." "Now, however, the fat is generally so suddenly solidified by a chilling process that crystallization is entirely prevented."

The illustrations of the cereal goods are admirably drawn, and we find the plan followed (which should have been adopted also in all cases in the first part) of giving the number of diameters to which the object has been magnified.

We strongly commend this handbook to the notice of all those possessing microscopes. Why should we wait until the public analyst tells us our food is adulterated? Why should not each one

of us turn private detective in this search for the honest trader? for it is quite certain the adulteration of food would soon come to an end if the perpetrators of the fraud were certain of being discovered.

MANCHESTER MICROSCOPICAL SOCIETY.—Annual Report and Papers read at the meetings. Manchester: Brook and Chrystal.

A *brochure* of 82 pages containing many interesting papers, most of which have already appeared in our pages. The members of this Society are to be congratulated upon the success of the Mounting Section, and their thanks are due to those gentlemen who have acted as demonstrators. It is a great aid to young students to be enabled *to see how* the various operations of section cutting, dissection and preparing generally, are performed by experts.

THE MIDLAND NATURALIST.—During the past few months there has been a very considerable improvement in the character of this journal, though the Editors will have to take care that their readers are not treated to too much of the principles of Biology. Mr. A. W. Wills has a very interesting article in the August number on "The Preservation of Native Plants," which should be read by all botanical students; it is nothing less than a sin to despoil localities of rare species, or even those not so rare, and it is satisfactory to know that the Management Committee of the Midland Union is taking the subject in hand.

Mr. W. B. Grove, B.A., has a paper on the Pilobolidæ, and Mr. J. E. Bagnall on the Flora of Warwickshire.

COLE'S STUDIES.—We wish we could see our way to induce a number of microscopists to subscribe for this useful work, which when bound up will form a very useful companion to the earnest microscopist. From a popular point of view we doubt the wisdom of splitting up the work into four distinct sections, the system was thus made elaborate and the continuity of the "newspaper" was broken, with, we think, but little advantage. We should have been inclined to think that the series would have been well supported by biological students, but Mr. Cole informs us that the reverse has been the case. Our own connection with microscopical literature has taught us the lesson that in this we must not look for either honour or profit, but Mr. Cole's work has been so well done and he has given so much for a very small subscription, that we are a little surprised his case should not be an exception to the rule.

We are afraid, as the case now stands, that there will be but little inducement for Mr. Cole to continue his "Studies" in microscopical science, but we hope ere this series is completed more subscribers will be found to make the venture pecuniarily successful.

The Microscopical News

AND

NORTHERN MICROSCOPIST.

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ON THE DETECTION OF SEWAGE CONTAMINATION BY THE USE OF THE MICROSCOPE, AND ON THE PURIFYING ACTION OF MINUTE ANIMALS AND PLANTS.

By H. C. Sorby, LL.D., F.R.S.

BY studying with the microscope the solid matters deposited from the water of a river, the previous contamination with sewage can usually be detected without any considerable difficulty. If the amount be serious, the characteristic particles of human excrement can easily be seen; and even if it is small, and has been carried a long way by the current, it can usually be recognised by means of the hairs of oats derived mainly from the droppings of horses, which resist decomposition for a long time, and are not consumed as food by minute animals. I, however, do not propose to enter into detail in connection with this part of my subject, but specially desire to call attention to the connection between the number of minute animals and plants, and the character of the water in which they live, and also to their influence in removing organic impurities.

For some time past I have been carefully ascertaining the number per gallon in different samples of river and sea water, of the various small animals which are large enough not to pass through a sieve, the meshes of which are about $\frac{1}{200}$ part of an inch in diameter. The amount of water used varies from ten gallons downwards, according to the number present. By the arrangements used there is no important difficulty in carrying out the whole method in a satisfactory manner. I confine my remarks entirely to general mean results.

The chief animals met with in fresh water are various entomostraca, rotifera, and the worm-like larvæ of insects. I find that the number per gallon and per-centage relationship of these mark, in

a most clear manner, changed conditions in the water, the discharge of a certain amount of sewage being indicated by an increase in the total number per gallon, or by an alteration in the relative numbers of the different kinds, or by both. All my remarks apply to the warm part of the year, and not to winter.

It is known that entomostraca will eat dead animal matter, though probably not entirely dependent on it. I have myself proved that they may be kept alive for many months by feeding them on human excrement, though they soon died without it. If the amount of food in any water is small, not many of such animals can obtain sufficient; but, if it be abundant, they may multiply rapidly, since it is asserted that in one season a single female cyclops may give rise to no less than four thousand millions of young. In stagnant muddy ponds, where food abounds, I have found an average of 200 per gallon. In the case of fairly pure rivers the total number of free-swimming animals is not more than one per gallon. I, however, found that where what may be called sewage was discharged into such water the number per gallon rose to twenty-seven, and the per-centage relationships between the different groups of entomostraca were greatly changed. In the Thames at Crossness, at low water, the number was about six per gallon, which fell to three or four at Erith, and was reduced to less than one at Greenhithe.

There is, however, a very decided limit to the increase of entomostraca when the water of a river is rendered very impure by the discharge of too much sewage, probably because oxygen is deficient, and free sulphide of hydrogen present. Such water is often characterised by the great number of worm-like larvæ of insects. Thus, in the Don, below Sheffield, in summer, I found the number, per gallon, of entomostraca only about one-third of what it is in pure waters; whilst, on the contrary, the number of worm-like larvæ was more than one per gallon.

Now if the minute free-swimming animals thus increase when a certain amount of sewage supplies them with ample food, it is quite obvious that they must have a most important influence in removing objectionable impurities. The number of the excrements of entomostraca in the recent mud of such rivers as the Thames is most surprising. In one specimen, from Hammersmith, I found that there were more than 20,000 per gallon; and the average number at Erith, in August, in 1882, was above 7,000, which is equivalent to about 200,000 per gallon of water at half ebb, from the surface to the bottom. This enormous number must represent a very large amount of sewage material consumed as food; and though, as in the case of larger animals, a considerable part of their excrements no doubt consists of organic matter capable of putrefaction, yet there can be no less doubt that the

amount entirely consumed in the life processes of the animals is also great.

As named above, I kept cyclops alive for many months by feeding them on human excrement. It is thus easy to understand why, when they abound in the Thames, the relative amount of human excrement is very considerably less than in the winter, when their number must be much smaller.

We thus appear to be led to the conclusion that when the amount of sewage discharged into a river is not too great, it furnishes food for a vast number of animals, which perform a most important part in removing it. On the contrary, if the discharge be too great it may be injurious to them, and this process of purification may cease. Possibly this explains why in certain cases a river, which is usually unobjectionable, may occasionally become offensive. It also seems to make it clear that the discharge of rather too much sewage may produce relatively very great and objectionable results.

Though such comparatively large animals as entomostraca may remove much putrefiable matter from a river, we cannot suppose that, except incidentally, they remove such very minute objects as disease germs, but it would be a subject well worthy of investigation to ascertain whether the more minute infusoria can, and do consume such germs as a portion of their food. If so, we should be able to understand how living bodies, which could resist any purely chemical action likely to be met with in a river, could be destroyed by the process of minute animals. Hitherto I have had no opportunity for examining this question critically, but have been able to learn certain facts which, at all events, show that it is well worthy of further examination. It is only during the last month that I have paid special attention to the number of the larger infusoria, and various other animals of similar type, met with per gallon in the water of rivers and the sea, which can be seen and counted by means of a low magnifying power. At low water in the Medway above Chatham, in the first half of June, the average number per gallon has been about 7,000, but sometimes as many as 16,000. Their average size was about $\frac{1}{1000}$th inch. Possibly the number of still more minute forms may be equally great; but, even if we confine our attention to those observed, we cannot but conclude that their effect in removing organic matter must be very considerable; and judging from what occurs in the case of larger animals, those $\frac{1}{1000}$th of an inch in diameter may well be supposed to consume as food, particles of the size of germs. Up to the present time I have, however, collected so few facts bearing on this question that it must be regarded merely as a suggestion for future inquiry.

So far, I have referred exclusively to the effect of animal life.

Minute plants play an important part in another way. The number per gallon of suspended diatoms, desmids, and confervoid algæ is, in some cases, most astonishing, and they must often produce much more effect than the larger plants. As far as I have been able to ascertain, their number is, to some extent, related to the amount of material suitable for their assimilation and growth. In the mud deposited from pure rivers their numbers is relatively small, but in the district of the Thames, where the sewage is discharged, I found that in summer their number per grain of mud at half-ebb tide was about 400,000, which is equivalent to above 5,000,000 per gallon of water. This is two or three times as many as higher up or lower down the river, and, out of all proportion, more than in the case of fairly pure rivers like the Medway. Their effect in oxygenating the water must be very important, since, when exposed to the light, they would decompose carbonic acid, and give off oxygen, under circumstances most favourable for supplying the needs of animal life, and counteracting the putrefactive decomposition so soon set up by minute fungi when oxygen is absent.

Taking then, all the above facts into consideration, it appears to me that the removal of impurities from rivers is more a biological than a chemical question; and that in all discussions of the subject it is most important to consider the action of minute animals and plants, which may be looked upon as being indirectly most powerful chemical reagents.

REMARKS ON PATHOGENIC BACTERIA.

By Dr. H. J. Detmers, F.R.M.S.

(*Continued from page 240.*)

ALL this, however, hardly affords a sufficient or satisfactory explanation as to the conditions necessary, on the one hand, to the exercise of the malignant properties of pathogenic bacteria, and on the other, to check the action of those malignant properties. I may here remark, I repeatedly found swine-plague micrococci, or, rather, diplococci, in the affected portions of the lungs and in the lymphatic glands of hogs that had recovered, or were recovering, from an attack of swine-plague. I found them four and five weeks after the morbid process had ceased to act, or, perhaps, more correctly, after a retrogressive process had set in. Consequently, those micrococci had become dormant, or had ceased to do mischief,

notwithstanding that inoculated into a healthy hog they produced the disease.

We must admit, we have as yet only made a beginning in our knowledge of disease-producing bacteria, and a great deal is yet to be learned and to be explored before the action of pathogenic bacteria, and the conditions necessary to develop, or to stop, respectively, their malignant activity, are fully understood. We have merely gained a peep into a new world, a world the existence of which our fathers had no knowledge of.

A few remarks I wish to make concerning antiseptics, or, rather, an internal use of antiseptic medicines. First, as to carbolic acid. It is a well-known fact that bacteria—pathogenic bacteria and others—are not killed by weak solutions of carbolic acid, or by such solutions as can with safety be given to an animal. But it seems that giving weak solutions of carbolic acid, of iodine, or of several other medicines, usually classed among the antiseptics, creates conditions in the animal organism unfavourable to bacteritic growth, and particularly to any action of the disease-producing properties of the pathogenic bacteria. At any rate, in my numerous (as I have not any of my notes at hand, I cannot state the exact number, which, however, may be learned from my reports to the Commissioner of Agriculture) experiments concerning swine-plague, I invariably found that an inoculation with swine-plague material—fresh lung exudations—failed to produce the disease, if the pig, as soon as inoculated, received every day for two weeks, or during the time which the period of incubation might have lasted, if the inoculation had been effective, a dose of carbolic acid in its water for drinking, amounting to ten drops of 95 per cent. carbolic acid for every hundred pounds of live weight. With small doses of iodine—iodide of potassium and iodine dissolved in water—I had the same results, but the iodine experiments were not continued, because the iodine solution proved to have otherwise a very unfavourable effect upon the animal organism. Some other antiseptics were tried, but the same proved either to be too expensive, or not sufficiently reliable to be of any practical value. Although the antiseptics (carbolic acid, iodine, etc.), administered in the water for drinking, invariably prevented a development of the morbid process, in some (few) cases the inoculation was followed by a slight reaction, consisting in a slight rise of temperature, sometimes preceded by a slight chill, and attended with more or less diminution of that natural vivacity peculiar to a perfectly healthy pig. In all those cases, however, in which the carbolic acid treatment was not commenced until the morbid process had fairly developed, or until important morbid changes had been produced, the antiseptic, beyond perhaps somewhat retarding the morbid process, had but little or no effect—did not save the animal's life. That in the

other cases, in which the carbolic acid, iodine, etc., treatment was begun immediately after the inoculation had been performed, the antiseptics were effective in preventing the outbreak of the disease, there can be no doubt, because in every one of my numerous inoculation experiments (see reports to the Commissioners of Agriculture), in which no antiseptics were used, every inoculation of a healthy pig with fresh lung exudation was invariably and in due time (usually in five to seven days) followed by a development of the very characteristic morbid process, notwithstanding that in all cases the inoculation was made in the ear with a small inoculation needle, with which not more than about one-fourth of a drop of fresh lung exudation was inserted. I may yet state, in all my inoculation experiments I endeavoured to use none but healthy pigs, and undiluted virus, or material direct from a diseased animal was inserted. I never used anything but perfectly fresh lung exudation. An inoculation with putrid lung exudation, or lung exudation swarming with *Bacterium termo*, may cause septicæmia, but will not produce swine-plague. Now, in what way, or by what changes produced, did carbolic acid prevent the development of the morbid process, or check the morbific action of the swine-plague bacteria? In the dilution given, it could not, and did not, directly kill them. If it had killed them, the slight reaction which, in a few cases, followed the inoculation, would have been hardly possible. Consequently, it must have produced changes, or created conditions, not suitable to the exercise of the malignant (pathogenic) properties of the bacteria. It, perhaps, prevented the formation of zoogloea-masses, something upon which the whole pathogenic power of the swine-plague bacteria seems to rest. A swine-plague micrococcus or diplococcus is abundantly small to pass with the greatest facility wherever a blood-disk is able to pass, consequently cannot cause any stagnation in the capillaries, but as soon as zoogloea-masses, often a great deal larger than a blood-disk, are formed, stagnation or partial stagnation in some of the finer capillaries, embolism, and in consequence exudation, and often extravasations of blood, processes which constitute the basis of all the morbid changes, are inevitable. This also explains why full-grown, vigorous animals, with firmer capillaries, and a strong heart-muscle, older boars for instance, often recover, where every younger pig is bound to die. Still, in what way the carbolic acid is acting, besides materially reducing the temperature of the animal, I will not attempt to explain.

To return to the southern cattle fever. I am sorry to say, an opportunity to make more extensive observations, and to verify by experiments those observations I have been able to make, has not been given me. I am working under instructions, and those instructions have to be obeyed. One thing, though, I may yet

mention, that is, I succeeded in several cases in checking the progress of the morbid process of southern cattle fever, which mainly develops in the liver, and (secondarily) in the spleen, by giving extraordinarily large doses of chinoidin (amorph. quinin), doses amounting to one drachm for every hundred pounds of live weight. A very marked reduction of temperature, about four degrees within twelve hours, regularly followed, and the disease abated.

In conclusion, Mr. President, allow me to say a word in regard to the classification of bacteria, schizophytes, or schizomycetes. As remarked before, we just stand at the threshold of what may be learned in regard to these minute and simple organisms, which will probably be considered in the future as a fourth kingdom, already named by Haekel, "Protista." Consequently our present classifications cannot be expected to be perfect. That of Cohn probably is the best, but it, too, can not lay any claim to perfection. So, for instance, it seems to me, the genus micrococcus has no real foundation, because according to my observations a micrococcus is not a perfect or mature bacterium (bacterium used as a generic term), but only a transitory or unripe form. Micrococci, wherever the conditions are favourable, soon form zoogloea-masses. In these zoogloea-masses the single cocci soon become double, form diplococci, then the glia breaks, whether on account of a consumption or absorption of the viscous mass, which constitutes the glia, by the cocci, or whether on account of the increased bulk of its contents caused by the growth of the cocci, that is, their change into diplococci, I do not know. The diplococci and the yet unchanged micrococci become free or gradually separated from the glia, but the development or propagation does not cease: on the contrary, proceeds with increased rapidity. The single cocci (micrococci), or most of them, soon become double and every diplococcus in a short time doubles itself at both ends, and forms a small chain, first of 4, then of 8, then of 16, etc., links; then the chains, when grown to some length, commence to separate, or to break up into shorter chains, or even into diplococci, but *never* in smaller portions than a double or figure ∞ micrococcus (diplococcus). At least I never saw a single micrococcus separate itself from a chain, or from a diplococcus, consequently the single micrococci must have a different source, and must proceed from so-called lasting spores, *which sometimes can be found developed among some diplococci that have become separated from a diplococcus chain.* Consequently, I have come to the conclusion that at least some micrococci—whether all the different species of micrococci, I do not know—constitute only a transitory form, and therefore have no right to be looked upon as the typical form of a genus.

Mr. President, inasmuch as you asked me to address this learned assembly totally unprepared, I hope the latter will treat with

indulgence my rambling talk. I admit and explicitly state, some of the hints given, for as such I wish to have them considered, are almost entirely based upon clinical observation, and some of them are as yet insufficiently supported by such absolute proof as is required in scientific research. But if the same give an impulse, or point out the direction in which experiments and further research may lead to important results, I shall not regret that I yielded, unprepared as I am, to your request.—*Journal of the American Society of Microscopists.*

POND LIFE IN WINTER.

NOTICING some observations on the above subject, and being myself for many years an ardent 'pond man," I will, with your kind permission, give an instance or two of my success in this little worked-out line of biology (especially in America) as regards pond hunting in the winter months. As far as my records go I have found very little difference. I am generally as successful on a winter's day as in the summer time, for in the hot weather you may go to pool after pool, ditch after ditch, and find nothing but thin mud perhaps, the result of continued drought. Then only fairly large lakes or reservoirs can be of any service, and these are generally very low and muddy round the borders and require some care, or else up to your knees you go unless you have a good drag to send across. Last winter I thought to venture out for some material for study, and I visited a large reservoir not many miles from Birmingham. The ice was very thin on account of a slight current. I dragged that pool, and that one haul sufficed to keep me fully employed for a month or more. I will briefly state what I found in that one haul on the weeds, *Anacharis* and *Myriophyllum.*

1st. That beautiful compound creature *Dendrosoma radians;* there were many, very many, large colony stocks. I saw several much larger than the one figured in Saville Kent's Manual, with the ciliated embryos tolerably plentiful in the water swimming about. I took a fine gathering at the time to Mr. T. Bolton. Also *Trichophrys epistylides*, *Podophrys cyclopeum*, *Acineta grandis*, a new species, *A. mystacina*, *A. cemmuarum*, *Raphidiophrys elegans*, *R. pallida* Schultze, *Actinophrys sol* and *Eichhornii*, *Stichotricha remex*, *Arcella vulgaris*, *dentata*, *aculeata*, *Amœba villosa;* also *Stephanoceros Eichhornii*, very plentiful and very large; *Melicerta ringens*, *Cephalosiphon limnias*, *Limnias ceratophylli*, *Vaginicola*, *Œcistes*, *Cothurnia*, *Epistylis digitalis*, *Opercularia*, *Floscularia*

regalis, *ambigua*, *ornata*, three species of *Anurea*, three of *Brachionus*. I have also generally managed to find *Volvox* in the winter in one pool or another, frequently under thick ice. Some years ago, I found the most beautiful gathering I ever found of *Volvox* under the ice. They were very large and plentiful, showing the beautiful yellow encysted, or, as it is called, 'resting stage.' The *Dendrosoma* I had under the microscope for weeks, hoping to be able to see for myself what is spoken of by Mr. Levick in the Manual, the gentle process of multiplication, but so far have not been successful. I generally go pond hunting twice a week. I frequently walk 15 or 16 miles where there is no kind of railway, and in all weathers, and so far as my experience goes I like winter almost, if not quite, as well as summer; for a long journey with a lot of glass bottles and other apparatus fags one so. I can fully agree with your remarks in your JOURNAL regarding the way in which this kind of study is neglected in America, as it appears to be. I think the microscopists in England are much more devoted to pond life on the whole than the Americans appear to be, and in such a glorious hunting ground, too, as, in my opinion, is pretty well proved by the beautiful work just published on Desmids of the United States by Mr. Wolle. I am truly delighted with it, as it contains so many examples in addition to Ralf's work. I have recently discovered *Xanthidum antilopœum*, *Arthrodesmus Incus*, *Staurastum Sieboldi*, *Cosmarium pseudonitidulum*, and a few very rare forms in a sunken pool.

In conclusion, I often regret that there are so many microscopists who neglect this useful, healthful, and fascinating branch of study. Do any of the American microscopists keep aquaria? I have kept aquaria of one kind or another for over twenty years, but only the last four years for the microscope, and have tried *Vallisneria*, *Anacharis*, *Nitella*, *Chara vulgaris*. For breeding rotifers, and for harboring them, nothing, I find, comes up to *Chara vulgaris*. The tube-dwelling rotifers love to build their habitations in the axils of the whorls and in close proximity to the beautiful red bunches of fruit; this plant is better even for this purpose than *Nitella*, and that is also very good. I have at present about twenty tolerably large aquaria in my room, all with *Nitella* and *Chara* in abundance, and fairly covered with *Melicerta ringens* and *M. tyro*, *Stephanoceros*, and many others too numerous to mention here.—*A. M. M. J.*

E. H. WAGSTAFF.

THE RELATION OF APERTURE AND POWER IN THE MICROSCOPE.

By Professor E. Abbe, Hon. F.R.M.S.

(Read 14th June, 1882).

II.—The Rational Balance of Aperture and Power.

(ii.) Division of the Entire Process of the Microscope between Ocular and Objective.

(Continued from page 232.)

THE conclusion from the foregoing experimental facts must therefore be:—

In order to obtain the best possible conditions for the utilization of the delineating capacity of any aperture, the focal lengths of the objectives must be sufficient to yield those powers which are necessary for distinct vision of the least details, with no greater super-amplification than is indicated by the critical valves of v defined above.

We shall therefore arrive at the point which is the aim of the whole discussion—the determination of the proper focal lengths for the various apertures—by tracing the practical inferences from this principle.

(1) The maximum apertures of the various systems—dry, water-immersion, homogeneous-immersion (for crown glass)—which are fit for ordinary use, may be approximately assigned by the numbers

$$a = 0 \cdot 90 \qquad a = 1 \cdot 20 \qquad a = 1 \cdot 35$$

because apertures which approach the ultimate limit of any system by less than about 10 per cent. cannot at all events be satisfactorily used for regular scientific work. The critical valves of v for these apertures may be put, as has been pointed out,

$$v = 4 \qquad v = 4 \qquad v = 6$$

The total powers which are *necessary* for the proper utilization of the same apertures are shown by the fourth column of the first table (Vol. II. 1882, p. 463), inasmuch as no observer of normal eyesight will be able to recognize *distinctly* details under a smaller visual angle than 2′ of arc. Adopting the figures of the table in round numbers, we obtain therefore the normal amplification [N] which is required for the wide-angled objectives of the various systems—

$$\frac{480}{4} = 120 \qquad \frac{640}{4} = 160 \qquad \frac{720}{6} = 120$$

and consequently the focal length $\left(f = \frac{l}{[N]}\right)$

2·1 mm. = 1-12th in. 1·56 mm. = 1-16th in. 2·1 mm. = 1-12th in.

According to the views developed above, objectives of these short focal lengths cannot be dispensed with, under the *present* conditions of microscopical optics, for those lines of scientific work in which it is of importance to obtain the *best possible* quality of the image (sharpness, definition), viz. such a degree of dioptrical perfection of the image as is not perceptibly inferior to the naked-eye vision of real objects, even on sensitive preparations.

As has been previously observed, higher powers than are strictly necessary for exhausting the attainable apertures, are desirable, and even indispensable, for many particular purposes. These may be obtained satisfactorily by using higher super-amplifications with the same objectives. Inasmuch as in that case the aim is merely to enlarge the image without displaying *new* details of the objects, the increase of the dissipation-effects with increasing v is not a serious drawback, because the visual angle of the minutest detail is increased in the same proportion. Though the absolute precision of the image will be diminished under the higher v, the *relative* will remain the same, and must still be sufficient for the more enlarged image if it was sufficient for the less enlarged. Now as *twice* that super-amplification which raises the defects of the systems just up to the threshold of vision, is always borne by objectives without any considerable or objectionable loss of definition, the increase of the ocular-power alone will be sufficient for reaching the *upper* limit of generally useful amplifications for the various apertures which is shown by the fifth column of Table I. Nevertheless it may be desirable that such higher powers should be occasionally obtainable under still more favourable conditions, that is with the lowest figure of v. I admit, therefore, that objectives of shorter focal lengths—down to *half* the values assigned above at the utmost—may still be useful for the *immersion*-apertures. (Not for *dry* lenses, because it would be decidedly irrational to force such high amplification from apertures which leave the delineating power much below the attainable limit).

On the other hand, it is quite certain that even the minimum powers which are required for the utilization of the said apertures, may be obtained under much higher super-amplification with a sufficiently satisfactory quality of the image, when the *utmost* degree of perfection is not required or when objects of less sensibility, *e.g.*, diatoms, are in question. Twice the critical value of v (*i.e.* 8, 8, 12, for the three systems respectively) will, however, be the limit in regard to objectives intended for somewhat general

application, and $2\frac{1}{2}$ (*i.e.* 10, 10, 15 respectively) the utmost admissible figure in regard to lenses for diatom work (the minimum amplifications, 480, 640, 720, being regarded); because if still higher ocular-powers should be required even for these minimum amplifications the deterioration of the image, attendant upon the enlargement of the aberration-circles, will become so perceptible, even with the least sensitive objects, that satisfactory recognition of the minutest details must be unqestionably lost. Though the details which are within the reach of the aperture may still be *seen*, the quality of the image will be so much inferior to that obtained by higher objective-powers and lower ocular-powers that it is obviously unwise to obtain under unfavourable conditions what may *as easily* be otherwise obtained. I must therefore consider as irrational constructions all those wide-angled lenses which do not yield even the lowest total power required for proper utilization of the aperture, except by a still greater amount of eye-piecing than is assigned above.

With these various concessions to personal customs and to particular purposes, the principles established here appear to be reconcilable with a rather wide latitude in their practical application. The *normal* focal lengths for the wide-angled objectives of the three systems being taken as above, we have the admissible maximum values of f (or minimum powers):

Dry.	Water-immersion.	Homogeneous-immersion.
5·2 mm. $= \frac{1}{4 \cdot 8}$ in.	3·9 mm. $= \frac{1}{6 \cdot 4}$ in.	5·2 mm. $= \frac{1}{4 \cdot 8}$ in.

And the minimum values of f (or maximum powers):

2·1 mm. $= \frac{1}{12}$ in.	0·78 mm. $= \frac{1}{32}$ in.	1·05 mm.. $= \frac{1}{24}$ in.

(2) As to those objectives which do not aim at the attainable maximum of delineating power, the discussion may be confined to the dry lenses, because in practice the other systems are not in use with much lower apertures (which is of course very prudent). In order to determine now the proper gradation of the focal length for the lower apertures of the dry system, the figures of ν must be determined which correspond to the different apertures. These values, as has been pointed out above, depend on many circumstances besides the aperture—particularly on the type of construction and (in the higher and medium apertures) the ratio of the working distance to the focal length. It will be impossible, therefore to assign values of ν which could claim a general acceptance, even for one standard of estimation. Any definite aperture (with

the exception of very low ones) will admit of a higher super-amplification if realized in a triple system than in a system of two lenses only, and at the same time (once more with that exception) of a still higher one, when the system is made with relatively short working distance, the best possible constructions being always supposed. In order to obtain any numerical data at all, I must therefore confine myself to a few particular kinds of objectives, which conform to those generally adopted standards of construction which may be considered as typical. With this view I have submitted to a careful trial *two* kinds of objectives: for the medium apertures, triple systems of about 0·50 aperture, with single plano-convex fronts and a working distance of about one-fifth of the focal length; for the small aperture, systems composed of two compound lenses with an aperture of about 0·15 and a working distance of about one-third of the focal length. Having compared a considerable number of specimens of both types of widely different origin (all being excluded which exhibited any defect of correction or technical construction), I found the critical super-amplification for the first type ($a = 0·50$) $\nu = 5$, and for the second type ($a = 0·15$) $\nu = 9$ in round numbers.*

Combining these values with the figure of ν assigned above for

PROPORTION OF APERTURE AND FOCAL LENGTH IN A NORMAL SERIES OF DRY LENSES.

Numerical Aperture, a.	Aperture Angle (air).	Total Power corresponding to a, N.	Critical Value of the Super-amplification, v.	Objective Power required, [N].	Focal Length required, f.
					mm.
0·10	11°·5	53	10·0	5·3	47·2
0·15		79·5	9·0	8·8	28·4
0·20	23·0	106	8·2	12·8	19·4
0·25		132	7·4	17·9	14·0
0·30	35·0	159	6·7	23·7	10·5
0·35		185	6·1	30·4	8·2
0·40	47·0	212	5·6	37·9	6·6
0·45		238	5·3	45·0	5·5
0·50	60·0	265	5·0	53·0	4·7
0.55		291	4·8	60·6	4·1
0·60	73·7	317	4·6	68·9	3·6
0·65		343	4·4	78·1	3·2
0·70	89·0	370	4·3	86·0	2·9
0·75		397	4·2	94·4	2·65
0·80	106·3	423	4·1	103·2	2·42
0·85		450	4·0	112·4	2·22
0·90	128·3	476	4·0	119·0	2·10

* The observations mentioned above were made several years ago, 1874-5. In the mean time nothing, however, has occured which can have changed the essential conditions in regard to the construction of dry lenses.

the apertures 0·85–0·90, we have obtained a numerical determination of *three* points of the series of apertures in the dry system; and we shall arrive at an approximate estimation for the intermediate points by interpolating the values of v between the said three points. The annexed tabular statement exhibits in the fourth column the series of v which results from the data given above, if the interpolation is made by means of a parabolic curve. The third column repeats the figures of the minimum total amplification N required for the various apertures, according to Table I.; the fifth column shows the objective-amplification [N] which results from the corresponding value of v; and the sixth column gives the focal length f which will afford that objective-power in every single case. Retaining fractions in the values of v and f, whilst the basis of the calculation is not determinate, has of course no other purpose than to prevent arbitrary leaps in the series of figures, which, according to the nature of the case, must show a continuous gradation.

According to the observations from which the figures of v have been derived, *triple* systems of moderate working distance must be supposed down to apertures of about 0·3, and *duplex* systems for all the lower ones which are considered here. The medium apertures, if realized with two lenses only, and the lower ones, if realized with single lenses, would show *considerably* smaller figures of v than are given above. On the other hand, triple systems with apertures much below 0·3 do not afford any perceptible increase of the admissible super-amplification—a fact which is well accounted for by the theory of the aberrations.

So far as the basis of my reasoning is admitted as valid, the table given above will exhibit the proper ratio of aperture to focal length in an *ideal* series of dry objectives of increasing apertures, traced out in *strict* conformity to the principle that every objective should yield, under the *best possible* conditions, such a total amplification as is *just* sufficient for fully exhausting the delineating power of its aperture—wherein "best possible conditions" means that no higher super-amplification, by tube and ocular combined, should be required than that which will *just* raise the dioptrical defects of the image up to the threshold of vision.

It is not my opinion that the *standard series* thus obtained should always be strictly adhered to in the practical construction of objectives. It is rather advanced here as a theoretical guide which will give a *general direction* in designing systems on a *rational* basis, but does not prohibit any deviation from that standard, provided it be justified by this or that practical consideration.

Deviations in the direction of *diminished* aperture (or increased power) need not be discussed here, since at present no tendency of that kind is met with, except in Microscopes of quite an inferior

class. The only question which deserves consideration is therefore, What latitude may be properly admitted for deviations from the standard proportion in the direction of *increased* apertures (or diminished objective-powers)?

The case here is somewhat different from that discussed above in the consideration of the proper utilization of the maximum aperture of any kind of system. For the angles which come into consideration now, being more or less within the attainable maximum, aperture is no longer difficult to attain. A surplus being easily obtained, may be sacrificed without hesitation, whenever such a benefit may be expected therefrom as is not counterbalanced by greater disadvantages. The question will therefore come to merely practical grounds: how far a surplus of aperture may afford a real (not only illusory) advantage, and what is the balance between these advantages and disadvantages which are perhaps attendant upon the increase?

As has been pointed out before (Vol. II. 1882, p. 469), there *is* a reasonable consideration which will recommend in some cases, viz. for the lower angled systems, the use of somewhat wider apertures than can be fully utilized by the total amplifications for which the systems are constructed, or—what is the same thing—will recommend the use of *lower powers* with a given aperture than is indicated by the corresponding figures of N of Table I. The surplus of aperture which is thus left unemployed in regard to the delineating power is utilized in promoting the illuminating power (or the brightness of the image), at least when narrow incident pencils are required for proper illumination of the objects. On the other hand, it has been shown that the said benefit is practically confined within some narrow limits.

As I have said already, I am fully aware of the uncertainty of the numerical data on which the above computations are based, which uncertainty will scarcely ever be overcome in a matter like that in question. Though in *my* opinion the figures advanced above will conform as nearly as possible to the present state of the Microscope, I should make no serious objection if any other observer arrived at figures which differ from mine by twenty or even thirty per cent., bearing in mind the interference of so many elements of individual judgment. I do not, therefore, lay any great stress upon the numerical details; the reader may try to improve them, or take them as a mere exemplification of general principles, illustrating their application to actual systems. What I insist upon is only that the theory of the Microscope is competent to indicate a distinct guide for the *rational* construction of objectives on the principle of proper economy of the independent capabilities of the systems (delineating power and amplifying power); that this principle leads necessarily to a certain propor-

tion between aperture and focal length; and that this proportion may be determined with such a degree of approximation as is required for a practical guide, at all events sufficient for showing the limits between rational and irrational aims.

In my opinion the question discussed here is of some general importance in regard to microscopy. It will, of course, do no harm that systems of lenses should be made of any design whatever and according to any particular taste, and full liberty must always be conceded in that respect. On the other hand, however, the Microscope has an important vocation as an aid to scientific research, and microscopical science is therefore fully justified in demanding that the prominent feature in the improvement of the instrument should always be to render it as useful as possible for its primary purpose, and that no hobbies of any kind should be permitted to take the lead in microscopical optics. In order to prevent this, and to secure progress in the direction of useful aims, the discussion of the question of the "rational" construction of objectives cannot be dispensed with.

(*Finis.*)

SOME EXPERIMENTS ON THE PHYSICS AND CHEMISTRY OF THE SAP OF PLANTS.

BY PROFESSOR JOHN ATTFIELD, Ph.D., F.R.S., F.C.S., ETC.

Read at Watford, 17th April, 1883.

HOW plants grow, whence comes their food, what is its nature, and how it is conveyed, are questions which have interested man in all ages. And to earnest, intelligent, unprejudiced truth-seeking enquirers answers to the questions have been obtained; answers general and incomplete at first, not even yet minute and perfect.

Burn a log of wood in a grate with due access of air. The wood and the oxygen of the air together yield carbonic-acid gas, vapour of water, and nitrogenous gas, all of which pass up the chimney; ash containing various mineral matters, which falls beneath the grate; and heat, which warms and comforts us. Conversely nature brings together heat, mineral matters, nitrogenous gas, water, and carbonic acid gas, and the products are wood and oxygen. To extract temporary comfort from the wood of our trees (or from coal, which is only altered wood), we convert that wood into warmth, smoke, and ash, and at the same time use up life-giving air. Bene-

ficent nature gathers together that warmth, and smoke and ash, and returns them to us in the form of trees and flowers, at the same time presenting us with the exact amount of life-giving air we had lost. How do plants grow? By additions of carbon, nitrogen, the elements of water, and certain mineral elements. Whence comes their food? From its storehouses, the atmosphere and the earth. How is it conveyed? By the carriers, air and water.

But these answers to the questions respecting the growth of plants, though very direct, are very crude and general. They represent only the beginning and the end of a vast number of processes. A log of wood on the one hand and the products of its burning on the other are only the terminals of two long series of substances, an analytical series beginning with the wood and ending with gases and ashes, and a synthetical series beginning with gases and ashes and ending with the wood. Reduce the wood to its elements, not by the rough and rapid method of free combustion, but by controlled heat in retorts, as at gas-works, and you will obtain a series of substances which includes scores of interesting materials—acids, spirits, colour-yielding bodies, illuminating agents, etc. Conversely, nature, between the constituents of the air or of the soil on the one hand, and the finished tree or flower on the other, forms a series of substances which includes scores of interesting materials—acids, perfumes, colours, flavours, starch, sugar, oil, etc.

In these two series or chains the products are related to each other or linked together, but in what way we do not yet perfectly know. Indeed, as to the form or character of many of the links themselves we as yet know nothing. But chemists discover fresh links every month, and chemistry is revealing to her students some hints as to the manner in which they are joined. So that man's information on these matters is increasing year by year, and what is revealed gives great encouragement to further research.

More fascinating, possibly, than the study of the chemistry of plant-growth, either from the analytical or from the synthetical points of view, is the study of the natural philosophy, as it used to be termed, or physics or true science of the subject; that is to say, the study of the cause of the various effects, or a consideration of the explanation of the effects. Bring a log of wood and air together under proper conditions and wood is reproduced, but one of the indispensable conditions here is that heat must be absorbed, the sun must shine on the leaves of the tree producing the wood. What happens to the heat that is absorbed before it again shows itself when the log is, in the grate, once more resolved into its elements? Does it lie dormant in the wood, latent, and for the time useless? Matter is always in motion; surely force is never quiescent. Neither matter nor force can suffer destruction, but each is ever

altering its form. It is certain that the elements of which wood is formed are undergoing ceaseless alteration during the growth of the wood before combustion re-converts the wood into those elements; it is equally certain that the heat which is absorbed by plants is doing ceaseless work before it reappears as heat when the combustion takes place. Nothing in nature is useless, nothing in nature is still.

The chemistry and the physics of plant-life at no time more forcibly and unitedly arrest the attention than when one considers the character of the watery fluid or sap of plants, what it is, its functions, and how it performs those functions. For in the sap of the plant-cells the water itself and the mineral matters brought up from the roots, and the gases brought in from the air, meet and unite. In the sap is commenced the structure which when perfected forms the leaf, the flower, the fruit, the whole edifice of usefulness and beauty.

Now, knowing something of chemistry and of physics myself, and having never lost my love for botany since the pleasant days of my studentship in that science some thirty years ago, under my present colleague Robert Bentley, I recently felt my interest in sap strongly aroused by the sight of some literally raining on me from a wounded tree growing in my own garden at Watford.

On the evening of the 3rd of April, beneath a white birch I noticed a very wet place on the gravel path, the water of which was obviously being fed by the cut extremity of a branch of the birch about an inch in diameter and some ten feet from the ground. I afterwards found that exactly fifteen days previously, namely, on March 19th, circumstances rendered necessary the removal of the portion of the bough which hung over the path, 4 or 5 feet being still left on the tree. The water or sap was dropping fast from the branch, at the rate of 16 large drops per minute, each drop twice or thrice the size of a "minim." Neither the catkins nor the leaves of the tree had yet expanded. I at once decided that some interest would attach to a determination both of the rate of flow of the fluid and its chemical composition, especially at such a stage of the tree's life. For although a good deal is already known respecting the "bleeding" of trees and the general character of the exuding fluid, very much remains to be discovered. Indeed, I could scarcely myself hope to do more than confirm some previous observers and perhaps give quantitative value in just one or two directions to the qualitative experiments of others. Thus, that the birch readily yields its sap when the wood is wounded is well known. Phillips, quoted by Sowerby, says:

"Even afflictive birch
Cursed by unlettered youth, distils
A limpid current from her wounded bark,
Profuse of nursing sap."

And that birch-sap contains sugar is known, the peasants of many countries, especially Russia, being well acquainted with the art of making birch wine by fermenting its saccharine juice.

But after searching two or three large libraries of scientific societies I could not find any hourly or daily record of the amount of sugar-bearing sap which can be drawn from the birch, or that of any sap from any tree, before the tree has acquired its great digesting or rather developing and transpiring apparatus—its leaf-system; nor could I meet with any extended chemical analysis of sap, either of the birch or other tree.

But to proceed with a description of the experiments. A bottle was so suspended beneath the wound as to catch the whole of the exuding sap. It caught nearly 5 fluid ounces between eight and nine o'clock p.m. During the succeeding eleven hours of the night 44 fluid ounces were collected, an average of 4 ounces per hour. From 8.15 to 9.15 on the morning of the 4th, very nearly 7 ounces were obtained. From 9.15 to 10.15, with bright sunshine, 8 ounces. From 10.15 until 8.15 in the evening the hourly record kept by my son Harvey showed that the amount during that time had slowly diminished from 8 to a little below 7 ounces per hour. Apparently the flow was faster in sunshine than in shade, and by day than by night. The flow was observed from time to time for nearly a week, the rate mentioned being maintained. The wound was open altogether for 21 days, hence probably rather more than 17 gallons of sap exuded during 20 days of that time; for my excellent gardener, Jonathan White, assured me that the tree had been "bleeding" at about the same rate for fourteen of the fifteen days that elapsed before the matter came under my notice, the first day the branch becoming only somewhat damp.

It would seem, therefore, that this slender tree, with a stem which at the ground is only 7 inches in diameter, having a height of 39 feet, and before it has any expanded leaves from whose united surfaces large amounts of water might evaporate, is able to draw from the ground about 4 litres or seven-eighths of a gallon of fluid every twenty-four hours. That at all events was the amount flowing from this open tap in its water-system. Even the topmost branches of the tree did not become during the three weeks abnormally flaccid, so that presumably no drainage from the upper portion of the tree had been taking place. Besides, after due calculations, I find that the amount of fluid which would exude in the three weeks would be greater than could be contained in the whole of the trunk and branches above the wound even if they were hollow. For three weeks, therefore, the tree had been drawing, pumping, sucking—I know not what word to use—nearly a gallon of food daily from the soil in the neighbourhood of its roots. This soil had only an ordinary degree of dampness. It was not wet, still

less was there any actually fluid water to be seen. Indeed, usually all the adjacent soil is of a dry kind, for we are on the plateau of a hill about 270 feet above the sea; the level of the local water-reservoir into which our wells dip is about 80 feet below the surface, and the subsoil is very porous; so that, altogether, the welcome rains sink away from our garden-earth rather rapidly. The total rainfall from the 19th of March to the end of the month was, our Secretary tells me, eight-tenths of an inch, a little over eighty tons per acre. No rain had fallen in April, when, on the 9th, my gardener, with some difficulty, so closed the wound as to stop the outflow. During the earlier part of the time we had frosts at night, and sunshine but with extremely cold winds during the days. At one time the exuding sap gave, I am told by two different observers, icicles a foot long. A much warmer, almost summer, temperature prevailed afterwards, and no wind. On the 4th of April the temperature of the sap as it escaped was constant at 52° F., while that of the surrounding air was varying considerably.*

The collected sap was a clear, bright, water-like fluid. After a pint had stood aside for twelve hours, there was the merest trace of a sediment at the bottom of the vessel. The microscope showed this to consist of parenchymatous cells, with here and there a group of the wheel-like cells which botanists, I think, term sphere-crystals. The sap was slightly heavier than water, in the proportion of 1005 to 1000. It had a faintly sweet taste and a very slight aromatic odour.

Chemical analysis showed that this sap consisted of 99 parts of pure water with one part of dissolved solid matter. Eleven-twelfths of the latter was sugar. Besides sugar, which occurred in this sap to the extent of 616 grains, nearly an ounce and a half, per gallon, there were present a mere trace of mucilage; no starch; no tannin; $3\frac{1}{2}$ grains per gallon of ammoniacal salts yielding 10 per cent. of nitrogen; 3 grains of albumenoid matter yielding 10 per cent. of

* In 'Nature' for April 5th, 1883, Mr. F. M. Burton, writing, on March 28th, from Highfield, Gainsborough, says: "A remarkable instance of the strong up-rush of sap in trees at this time of the year occurred here during the late severe weather. The boughs of a sycamore overhanging a road were trimmed on the 21st of March during a very keen frost, and next day icicles of frozen sap, varying in length from a couple of inches to a foot, were hanging from the severed ends. The icicles were semi-opaque in appearance and slightly iridescent, like the sheen on the moonstone, and, when put in a bottle and melted, the product was pure sap. The sycamore, being one of the earliest trees to develop leaves, had its sap rising, notwithstanding the intense cold and late season; while a beech, which is much later in coming out, and an ash, which is usually latest of all, whose boughs had also been lopped, showed no signs of bleeding, and the cuts remained dry and bare. The icicles have been melted, reformed, and melted again since the 21st, and still the sap is dropping from the cuts."

nitrogen; a distinct trace of nitrites; 7·4 grains of nitrates containing 17 per cent. of nitrogen; no chlorides, or the merest trace; no sulphates; no sodium salts; a little of potassium salts; much phosphate and organic salts of calcium; and some similar magnesian compounds. These calcareous and magnesian substances yielded an ash when the sap was evaporated to dryness and the sugar and other organic matter burnt away, the amount of this residual mineral matter being exactly 50 grains per gallon. The sap contained no peroxide of hydrogen. It was faintly if at all acid. Exposed to the air, it soon swarmed with bacteria, its sugar being changed to alcohol, and this again in a few days to acetic acid. The birch-sap had changed to birch-wine and then to birch-vinegar.

It is noteworthy that the sap when drawn contained a ferment capable of transforming starch into sugar. Two former students of mine, Messrs. Dunstan and Dimmock, who have devised a method of ascertaining the power of such ferments, were good enough to apply their process to some of my sap. They found that one gallon would convert into sugar 21 grains of dry starch, the latter being first gelatinised. Here probably we get an idea of the methods which nature employs in converting one substance into another during the synthetical process or growth, and during the analytical process or more or less rapid decay or combustion of plants. Future investigation in physiological botany will doubtless include much experimenting with ferments. It is an extremely interesting branch of study. The action is well illustrated in the conversion of starch into gum, sugar, etc., during the germination of barley in the manufacture of malt.

Shortly, respecting the composition of this sap, I may state to the general members of a Natural History Society without risking the dignity of my subject, that a teaspoonful of sugar put into a gallon of such rather hard well-water as we have in our chalky district, would very fairly represent this specimen of the sap of the silver birch. Indeed, in the phraseology of a water-analyst, I may say that the sap itself had 25 degrees of total, permanent hardness.

How long the tree would continue to yield such a flow of sap I cannot say. Probably until the store of sugar it manufactured last summer to feed its young buds this spring was exhausted. Even within 48 hours the sugar slightly diminished in proportion in the fluid, the specific gravity going down from 1004·92 to 1004·38.

And now with regard to the physics of the matter. What causes this outflow? Or, to put the larger question at once, what causes the rise and general movement of sap in plants—a movement which extends from the lowest rootlet to the topmost leaf or twig?

The movement of fluid in plants has been set down to atmospheric pressure, to capillary attraction, to endosmotic action, and to the indirect influence of wind and warmth on elastic tissues.

Atmospheric pressure, however, will only sustain a column of water to a height of about 34 feet, as seen in the case of a common pump, whereas the height of my birch is 39 feet, and the top of a tree is often scores and sometimes hundreds of feet from the ground.

By capillary action water ascends in wetted tubes as narrow as hairs (*capillus*, a hair) when they dip into water; and the height attained above the level of the water outside the tube, other things being equal, is inversely proportional to the diameter of the tubes. Thus, at summer temperatures, in a wetted tube one twenty-fifth of an inch in diameter, water will rise about an inch and a quarter. Such action is apparently insufficient to account for the rise of sap in trees.

With regard to endosmose, Dutrochet found that a tied bladder containing a saline solution decreased in weight when placed in water, and that a tied bladder containing water increased in weight when placed in a saline solution. The saline solution passed through the wall of the bladder or cell in one direction quicker than water passed through it in the other direction. This action between a weaker and a stronger fluid he called endosmose. Each cell of a plant is the analogue of the tied bladder, and between one cell and another there will be the passage of fluid whenever the densities of the two fluids vary. This action proceeds from cell to cell throughout a plant, and hence may account for slow movements of fluids within plants. But the tips of roots do not necessarily dip into such an amount of water as would seem to be necessary were endosmose the prime cause of the ascent of sap. I could not discover after much careful search that the rootlets of my birch were in contact with actual fluid water. They seemed rather to terminate in minute moisture-laden air spaces. Besides, as I understand endosmose, its very existence depends on concurrent exosmose. Hence, the inflow of, say, common water to a plant containing its minute proportions of saline matters should be accompanied by an outflow of a fluid of different density, either absolutely pure water on the one hand, or, on the other, sap containing elaborated material such as sugar. I am not aware, however, that the outflow of either kind of fluid has ever been observed, or that any observer contends that it takes place. I cannot, at present, accept endosmose as a satisfactory and complete explanation of the outflow from the branch of my birch.

Herbert Spencer says that wind in bending twigs, branches, and trunks gives alternate squeezings to one side and the other and corresponding extensions of tissue on the opposite sides, and that this action sets up currents of sap within the tissue. I have great respect for this eminent and far-seeing sociologist, and I doubt not that wind exerts important actions upon plants, but my birch was

yielding nearly a gallon of sap daily when no wind was blowing, and the amount was not perceptibly altered when the wind did blow.

The suggestion has been made that the warmth of spring expands the solid parts of a plant, and that nature abhorring a vacuum drives in water to supply the thus enlarged space or spaces. How any such warmth expands the solid without at the same time expanding the liquid and gaseous contents of a plant is not stated. Moreover, this driving up of water or sap is only a fresh name for the pressure caused by the weight of the atmosphere, which pressure has already been shown to be insufficient to account for all the facts of the case.

"Root-pressure" is also a name which frequently occurs in the vocabulary of some writers. And a harmless name it is for describing some of the effects we are considering. But regarded as a cause I find it is only a fresh name for either atmospheric pressure or for endosmose.

Transpiration, that is, the evaporation which goes on from leaves, especially from their under surfaces, is said to be a cause of the rise of sap. It would be fairer to say, however, that rise of sap accompanies transpiration. For my birch when giving nearly a gallon of sap a day had neither leaves nor catkins upon it, and when I first noticed the outflow not a bud had burst. It follows, apparently, that there may be flow of sap in the absence of transpiration, hence that the one cause of the flow of sap is not transpiration. Doubtless transpiration plays an important part in the plant-growth, and, as I understand, is so active at certain times as to be the possible cause of such a reduction of pressure within a plant, as compared with external atmospheric pressure, that so far from any fluid being exuded from a cut branch at such times, water may even be strongly sucked in. At all events, so far as transpiration does affect the flow of sap, such part of the flow would still seem to be caused only by atmospheric pressure.

There remains only to be considered the enormously powerful attractive force termed the chemical force as lying at the bottom of the attraction of plant-tissue for sap. In a plant the molecules of carbonic acid, water, nitrogen-bearing bodies, and mineral substances are bound together by the chemical force into compounds, and these latter into more complex compounds, and so the substance of the plant or tree is formed. But the chemical force acts only when bodies are in contact, that is, at insensible distances from each other. How then can a root-tip obtain water or mineral matters? Water it will obtain from the molecules of water-vapour in contact with the tip. At the tip the molecules will coalesce to drops, and these will dissolve contiguous molecules of mineral matter. Then may come in capillary attraction, which is a variety

of non-chemical molecular attraction; then may come in any influence of atmospheric pressure whether set up by transpiration or otherwise; then may come in endosmose, which is indeed probably itself caused by the chemical attraction between molecules of water and of saline matter.

But the chemical force is static rather than dynamic, how then can it set up a current? By the attraction for each other of the components of tissue, on the one hand, and on the other, of those true compounds of water with even minute amounts of gases or solids which we know to exist. Tissue-compounds and water-compounds meeting, ordinary chemical changes occur, force, in the form of heat, is absorbed, more complex compounds are built up, and the great bulk of the water *per se*, or, it may be, water holding in solution effete matters, is left to shift for itself, perhaps even repelled, to escape in the direction of least resistance—by transpiration in summer and by slower processes in spring. The water exuding from my birch-branch may be such water, a portion of the water that might ordinarily escape from the external surfaces of the wood or otherwise, with the difference that it is carrying away matter (sugar, etc.) not wanted just now, perhaps, but which unfortunately, or, at all events, as we believe, will be wanted presently. The warrant for the remarks respecting the highly aqueous water-compounds is to be found in the known tenacity with which water retains traces of gases, as shown by Groves, and in the altered properties possessed by water when containing even traces of saline or other matters. Finally, whence comes the large quantity of chemical force or chemical attraction necessary for the binding together of such numbers and such amounts of substances as occur in the fully-formed wood of a forest tree? From the heat-force poured on to its leaves by the sun—force which will be re-converted into heat when the wood is transformed, no matter whether slowly by decay or quickly by combustion, into its constituent gases and ashes.

It would be unwise, however, to speculate further without those confirmations and checks which experiment alone could afford, and without those safe guides which experiment alone could furnish. Here as elsewhere in all departments of knowledge earnest seekers after truth are wanted, men possessing, for this enquiry, adequate knowledge of physics, chemistry, and botany, and with the necessary time and means for carrying on the work. To such men would certainly, sooner or later, be accorded the honour of discovering, or unveiling, one more of the laws by which all nature is governed. Then we shall know the true cause or causes of the movements of sap in plants.—*Herts. Nat. Hist. Soc.*

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

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TO OUR READERS.

IT is with much regret that we have to announce the suspension of publication of this Journal after the issue of our next, or December number. When "The Northern Microscopist" was first issued it was thought that such a periodical would be of use in recording the proceedings of the various Microscopical Societies in the North, and in aiding amateur microscopists generally. The gradually increasing paucity of such matter induced us to enlarge the scope of our work, and we thought that by partially discarding our limited title and taking up that of THE MICROSCOPICAL NEWS we should be opening our pages to those Societies who desired to keep a permanent and public record of their proceedings, where-ever they might be located. The interest evinced in such a scheme is far less than we anticipated, and as we came into existence mainly for this purpose, we feel it our duty to suspend publication.

We regret that circumstances compel us to come to this decision: we have nearly completed the fourth year of existence, and the Journal would, if more life had been put into it, perhaps a little more than paid its way; but we are now working under altered circumstances.

In the early days of "The Northern Microscopist," the Editor held the position of Government Inspector of Alkali Works, and the reading up of Microscopical literature, together with proof-reading, served to while away many a weary hour of railway travelling and waiting at stations for trains. Having now resigned this appointment, his time is so much occupied with his profession as Consulting Chemical Engineer, that the necessary attention cannot be given any longer to THE MICROSCOPICAL NEWS.

Microscopy has so many charms, when followed intelligently, that it is hoped the discontinuance of this Journal (or rather its *suspension*, for it *may be* continued later on should the Editor feel more at liberty), will not cause any beginners to give up the study.

The Editor desires to call attention to two works of special

merit, and suggests that the money hitherto devoted to subscription with us, might, with even better results, be invested in them. The first is "The Journal of the Postal Microscopical Society," which may be purchased from Mr. W. P. Collins for six shillings per annum; while the second is "Cole's Studies in Microscopical Science."

Mr. Cole, through want of support, was about to suspend the issue of these "Studies," when Messrs. J. G. Hammond & Co., of Edmund Street, Birmingham, came to the rescue, and intend to issue the letterpress, for one year at least, at their own risk. In their circular they say, "Although we shall in future produce and issue the work, it will, as heretofore, be edited by Arthur C. Cole, Esq., F.R.M.S., of Oxford Gardens, Notting Hill, London, and the high class preparations, solely of his production, together with the beautifully coloured Chromo-lithographs, both of which have called forth the highest encomiums from subscribers and press, will be continued exactly the same as in the preceding two volumes." The preparations to accompany the letterpress will, as heretofore, be prepared by Mr. A. C. Cole, and by Mr. Martin J. Cole, the Instructor in Practical Microscopy at the Birkbeck Institution.

We feel assured that no one meaning work, will be dissatisfied with either of these publications. We have extracted several articles for this month's number from the latter.

ON INJECTING.

By Arthur J. Doherty.

THE term injecting, in its micrological application, signifies the filling of the arteries, veins, and other vessels of animals with coloured substances for the purpose of showing their arrangement in, and their course through, the tissues.

Proficiency in the art of making anatomical injections can be acquired only by continued practice, and by the exercise of patience and perseverance; the same remark applies even more strongly to the injecting of diseased tissues which have been excised out of the living body, as in this case there are so many vessels, which have been severed, requiring to be ligatured to prevent the escape of the injection. The beginner is recommended to note carefully the causes of each failure, and to take precautions to avoid these in his subsequent practice. If this is done, the art of injecting will be learned sooner, and more easily than it otherwise could be.

There are three well-known methods of making injections, which may be distinguished as follows :—

(1) Injections made with a syringe;

(2) Mechanical Injections, in which the function of the syringe is replaced by the pressure of a column of water or mercury;

(3) Natural Injections; or the introduction of pigments into the circulation of living animals.

The natural method is generally resorted to in cases where the two preceding processes are either altogether impracticable or very difficult to perform; as, for example, the filling of the bile ducts throughout their course in the liver.

Injecting by the pressure apparatus is very convenient when time cannot be spared to do the work by hand; but the latter method is *the one* which should be mastered on account of its simplicity when once learned, and the ease with which it can be performed.

The substances used for making injections may be divided into two classes: one class includes all those which are fluid at the ordinary temperature; while the other class includes such as become fluid only when heated, and return again to the solid form on cooling; these are called "masses."

The following are the most useful injections :—

PRUSSIAN BLUE FLUID.

Glycerine	1 oz.
Methylated Spirit	1 oz.
Ferrocyanide of Potassium...	12 grains.
Tinct. Perchloride of Iron...	1 drachm.
Water	4 oz.

Mix together the glycerine, spirit, and water, and divide the mixture into two equal parts. In one part, dissolve the Ferrocyanide of Potassium (*a*), and to the other part add the Tincture of Perchloride of Iron (*b*); *b* must now be added very gradually to *a*, the mixture being well shaken after each addition of the iron solution. Keep this fluid in a stoppered bottle, and shake it well before using it.

TURNBULL'S BLUE.

Dr. Beale in his work on the microscope says, "My friend, Mr. B. Wills Richardson, of Dublin, has introduced Turnbull's Blue in preference to ordinary Prussian Blue. Ten grains of pure Sulphate of Iron are to be dissolved in an ounce of glycerine, or better still, in a little distilled water, and then mixed with glycerine, and thirty-two grains of Ferridcyanide of Potassium in another small proportion of water and the solution mixed with glycerine. These two solutions are then gradually mixed together in a bottle, the iron solution being added to that of the Ferridcyanide, and the

mixture ensured by frequent agitation." The deep blue fluid thus prepared must be added to one ounce of glycerine, one ounce of methylated spirit and four ounces of water, as in the Prussian Blue fluid. Dr. Beale considers these proportions unnecessarily large, and gives the following recipe, which, however, I have not myself found to answer well, the injection, especially under high powers, being too faint.

Ferridcyanide of Potassium... ...	10 grains.
Sulphate of Iron	5 grains.
Water	1 oz.
Glycerine	2 oz.
Alcohol	1 drachm.

The advantage of this injection over Prussian Blue is that the colour does not fade in course of time.

BRUCKE'S SOLUBLE PRUSSIAN BLUE.

A. Ferrocyanide of Potassium, 217 grammes dissolved in one litre of distilled water.

B. Perchloride of Iron, 10 grammes dissolved in two litres of distilled water.

C. A cold saturated solution of Sulphate of Soda.

Mix one part of A with one part of C (α), and mix one part of B with one part of C (β). Add β to α, and allow the mixture to stand about three hours (longer if necessary), collect the deposit on a filter. Wash the deposit three or four times a day for a week by pouring over it small quantities of distilled water. The washing must be discontinued as soon as the water which runs through is quite blue; and the powder thus prepared must be dissolved in distilled water and mixed with sufficient gelatine to form a firm jelly.

DR. BEALE'S ACID CARMINE FLUID.

Dr. Beale says, "After trying a great many different combinations, I arrived at the following, which answers the purpose exceedingly well:—

Carmine	5 grains.
Glycerine, with 8 or 10 drops of Acetic or Hydrochloric Acid...	½ oz.
Glycerine...	1 oz.
Alcohol	2 drachms.
Water	6 drachms.
Ammonia...	a few drops.

Mix the Carmine with a few drops of water, and, when well incorporated, add about five drops of liquor ammonia. To this dark red solution, about half an ounce of the glycerine is to be added, and the whole well shaken in a bottle. Next, very gradually pour in the acid glycerine, frequently shaking the bottle

during admixture. Test the mixture with blue litmus paper, and if not of a very decidedly acid reaction, a few drops more acid must be added to the remainder of the glycerine, and mixed as before. Lastly, mix the alcohol and water very gradually, shaking the bottle thoroughly after each successive portion till the whole is mixed. This fluid, like the Prussian Blue, may be kept ready prepared, and injections made very rapidly with it." This is, without doubt, one of the best fluid injections ever devised. It is particularly useful for injecting such lower forms of animal life as insects, shell-fish, snails, and small fishes.

Acetic Acid is to be preferred to Hydrochloric for the purpose of acidifying the solution. The object in adding acid to carmine injections is to precipitate the carmine, and so prevent it from transuding through the walls of the vessels into which it is thrown.

DR. CARTER'S CARMINE.

Carmine	60 grains.
Liq. Ammonia (B. P.)	180 minims.
Glacial Acetic Acid	86 minims.
Gelatine (Gel. 1 part, water 6 parts)	2 oz.
Water	4 oz.

The Carmine is to be dissolved in the ammonia and added to the water. This solution is added to one half of the gelatine, and to the remaining half of gelatine is added the acetic acid. The acidified gelatine solution is next mixed drop by drop with the portion containing the carmine, and the whole is filtered through fine flannel before use. To be successful in making this mass, it is necessary to use *glacial* acetic acid, and the *strong* liquor ammonia of the British Pharmacopœia.

A good supply of each of these injections should be kept ready for immediate use. It is convenient to keep the masses in vessels made either of block tin or copper, in order that they may be readily heated. They may, however, be kept in earthenware jars and melted by placing the jars in boiling water. The fluid injections should be kept in stoppered bottles, and the mouth should be sufficiently large to admit the nozzle of the syringe. The different injections should be filtered occasionally to remove any particles of matter which may get into them, and they should be distinctly labelled; this precaution will effect a saving in time and prevent mistakes.

THE SYRINGE.

In selecting a syringe, the following points should be attended to. (1) The syringe, which should be of at least one ounce capacity, should be furnished with two rings at its upper end, one on each side, for the fingers to pass through; (2) It should also be furnished with three pipes or canulæ of about $\frac{1}{16}$ in., $\frac{1}{32}$ in., and $\frac{1}{64}$ in.

diameter; and in order that they may be secured firmly in the vessels whilst making an injection, they should be provided with a pair of arms to pass the ligature round; (3) The piston should fit the cylinder so accurately that if the nozzle of the syringe be closed with the finger, and the piston be drawn up, it will, on being released, instantly return to its former position; (4) The syringe should be provided with a stopcock. A syringe of this kind costs about 15/-. If the beginner does not desire to go to so much expense, a glass syringe costing about 1/- may be used. The canulæ can be made out of glass tubing, by drawing it to a fine point in a Bunsen's flame, and then cutting off the part required.

ON DESTROYING THE LIFE OF AN ANIMAL INTENDED FOR INJECTION.

The life of an animal intended for injection is destroyed most easily and in the best manner by opening it from anus to throat, and cutting deeply into the heart across the right auricle. This is, of course, done whilst the animal is under the influence of chloroform, or even immediately after it has been suffocated by chloroform. To facilitate the bleeding, the animal should be suspended alternately by the hind and front legs, and as the blood coagulates in the wound in the heart, it should be removed. The best way to administer the chloroform is to place the animal in a box, drop in a piece of cotton wool saturated with chloroform, and close the lid. In from five to fifteen minutes the animal will be dead. Half an ounce of chloroform is quite sufficient to kill any cat, and the same quantity should suffice for a dog a foot high.

ON INJECTING A WHOLE ANIMAL.

A young animal is best for this purpose. A rabbit is, perhaps, the best subject for a beginner to select. After having killed it, immerse the body in hot water for about fifteen minutes; then take it out, pass a ligature round the aorta close to the heart, make a longitudinal incision in the aorta, and insert the canula of most suitable size. Bind the canula firmly in the artery, and attach the stopcock. Oiled worsted is the best substance which can be employed for tying the pipes in the vessels; it should not be drawn too tight or it will cut through them, and so permit the pipe to come out. *All* vessels must be opened longitudinally, *not* transversely, or they would probably contract so much as to exclude the possibility of making an injection.

A good supply of hot carmine mass (Dr. Carter's Carmine) should be ready for use. Fill first the syringe with the injection and then the stopcock and canula. Then insert the nozzle of the syringe into the stopcock, taking care that in doing this no air is admitted, or it will be forced into the vessels, and the passage of the fluid impeded. These points having been attended to, the injection should

commence. The amount of pressure exerted on the piston should at first be very slight; but it will be necessary to increase it as the injection proceeds. It is advisable to support the animal in the water either with the left hand, or to allow it to rest at the bottom of the vessel containing the water. The filling of the spleen should be watched carefully, and as soon as it is fully distended, more injection mass should be prevented from flowing into it by tying a ligature round its artery. The splenic artery is easily found. It arises as a branch of the cœliac axis, and enters the substance of the spleen at the hilus on its concave surface. In order to obtain a perfect injection of the kidney, it should be drained of all blood by opening the renal vein. Blood and carmine mass will at first flow out together; but as soon as the carmine flows out freely and unmixed with blood, the vein should be ligatured, and the vessels allowed to fill slowly. The injection may be considered complete when the transparent parts about both the upper and lower extremities show a reddened and slightly distended appearance. The internal organs, when well injected, have a deep red color, and appear as if inflated with air. In this operation, the lungs remain untouched by the injection, and they must therefore be injected separately through the pulmonary artery either *in situ*, or after they have been excised. In order to render the capillaries of the alveoli perfectly distinct in section, it is usual to distend the air cells of the lungs by pouring melted cocoa-nut oil down the trachea. The oil, on cooling, solidifies, and makes the cutting of extremely thin sections after hardening an easy matter. After the injection is completed, the open vessels should be tied, and the animal placed in cold water; half an hour afterwards, the different parts should be dissected out, and placed in methylated spirit.

In passing, it may be remarked that some histologists consider it preferable to inject the entire animal, not through the aorta, but through the carotid arteries; this operation is, however, much more difficult than the preceding one, and there is no real necessity for proceeding in this way when the former method yields such excellent results.

HARDENING INJECTED TISSUES.

Injected tissues must be hardened in spirit. After the first day's immersion, they should be transferred to fresh spirit for two more days, and then into fresh spirit again, and kept in this until ready for cutting into sections. It is *never* necessary to use absolute alcohol as a hardening agent; and it is seldom needful to place the tissues first in weak spirit and gradually to increase the strength up to perfectly anhydrous alcohol. The length of time required for hardening depends upon the kind of tissue, its size, and, to a certain extent, upon the quantity of spirit used; the smaller the size of the tissue, the more rapidly will the hardening be done.

Brain, kidney, and spinal cord, are rendered sufficiently hard for cutting into sections in three weeks; lung, liver, spleen, pancreas, the intestines, the tongue, etc., take a longer time, usually from five to eight weeks. A saturated aqueous solution of Picric Acid is sometimes used as a hardening agent; but its action as a very persistent yellow dye is much against its employment for this purpose.

INJECTING SEPARATE PARTS.

Having now described the method of injecting an entire animal, I shall pass on to an account of the modes of injecting different organs and parts of an animal.

The Kidney. On account of the comparatively large size of the blood-vessels of the kidney, it is a very suitable organ for the beginner to practise upon. The filling of the arteries should first be mastered, and afterwards the injecting of both the arteries and veins. To inject the arteries, tie the canula in the renal artery and throw in carmine mass or Prussian Blue. When the vessels are about half filled, the injection will begin to flow from the renal vein which should then be ligatured, and the vessels slowly filled. After this is done, place the organ in spirit or cold water to cause the jelly to set, and then cut it into pieces of a suitable size for hardening. A good preparation will show the medullary portion filled with long and slightly curved arteries running parallel to each other; the artery or afferent vessel entering the Malpighian tuft, the vein or efferent vessel leaving it, and the whole of the vascular portion of the kidney surrounding the Malpighian tufts filled with a network of arteries, capillaries and veins. If a double injection is required, inject the vein first with Prussian Blue or Turnbull's Blue, and afterwards the artery. The kidney of a rabbit is better than that of any other animal for making injected specimens.

The Liver. If the entire animal has been injected from the aorta, only the vessels supplied by the hepatic artery will be filled. In this case, the portal vein should be injected with one colour, and the hepatic vein with another.

The method of injecting the bile ducts in a satisfactory manner had long puzzled anatomists before Chrzonszczewsky introduced his method of natural injection. The animal having been chloroformed, a solution of indigo-carmine is to be introduced into the jugular vein. The dose should be repeated several times, and the animal killed in about an hour and a half. The other vessels should then be filled and portions of the liver hardened and cut into sections as already described.

The Spleen is injected either from the aorta, or from the splenic artery.

The Pancreas should be injected from the aorta. This organ requires to be well hardened before being cut into sections.

The Lung. The method of injecting the lung has been already described. A double injection is made by filling the veins first and the arteries afterwards. The veins should be handled very carefully, else, on account of their delicacy, they will be easily ruptured.

The Brain and Spinal Cord are injected from the aorta. The cord requires very careful manipulation to dissect it out of the vertebral canal in such a way as not to injure it. A pair of sharp bone forceps will be found very useful for cutting through the verterbræ.

The Tongue is injected through the carotid arteries.

The Stomach is injected through the gastric artery. The veins of the stomach are to be filled by opening the portal vein, and directing the point of the syringe towards the stomach.

The Intestines. The upper portion of the duodenum must be injected through the arteries which are derived from the cœliac axis; the lower part of the duodenum, and also the ileum, the cæcum, and the ascending and transverse colon must be injected through the arteries which are derived from the superior mesenteric artery. The descending colon, the sigmoid flexure, and the rectum are supplied with blood through arteries which originate in the inferior mesenteric artery, and must be injected through these arteries. In every case, if the vessels which pass to and from a part or an organ are large enough to admit the canula of the syringe, it is advisable to inject through them; but if a vessel will not admit the canula, inject through that vessel of which the other smaller vessel is a branch. The large vessel should of course be ligatured at points above and below that in which the pipe is inserted, or the injection will flow into adjacent parts.

The Lymphatics. Ludwig's puncture method is the simplest way of injecting the lymphatics. With a scalpel, make a slight incision in the pad of a dog's or cat's foot, and insert the nozzle of a hypodermic syringe, and inject Brücke's Blue into the pad. Withdraw the syringe, close the cut with the thumb, and draw the fingers along the limb. This will force the injection through the spaces in the connective tissue into the lymphatics.

A little practice will soon enable the beginner to overcome the difficulty which attends the injecting of arteries; but it will be found that the veins, on account of their thinness and delicacy, require much more careful manipulation. A few failures may be expected at first; but after three or four trials, much of the difficulty of injecting will disappear. Careful dissection and attention to the directions here laid down will save much labour and loss of time.

THE CRANE FLY (*Tipula Oleracea*).

FROM COLE'S STUDIES.

THE common Crane Fly or Daddy Long Legs, or as it is called in some parts of the country, Harry or Peter Long Legs, is a very well-known British insect. Its larva or grub indeed enjoys quite an unenviable notoriety, for the reason that the little brown, legless and wormlike creature no sooner emerges from the egg among the roots of a grass crop, or garden lawn, where it has been deposited by a careful parent, than it applies itself with an energy undisturbed by other cares to the one absorbing object of its life at this period. Its destiny is to eat, and eat it most assuredly does, with a degree of perseverance worthy of a better cause. So great is its voracity that the roots of the grasses, and other plants where Tipula-larvæ abound, are so completely eaten away by them that it has been found possible, over large areas, to roll up the withered turf as easily as if a turf cutter had been under it. Naturally, under these circumstances, the grub increases in size, and after a few months it passes into the quiescent state of a pupa, and then eats no more, but undergoes a series of metamorphoses, resulting in the development of wings, antennæ, or feelers, legs, and other organs, external and internal, and there emerges from the ground some fine autumn evening the perfect dipterous (Gr. dis, twice; pteron, wing) insect which is the subject of the present study.

In considering the anatomy of the crane fly, the first point to strike the observer is the division of its body into three distinct regions—head, thorax or chest, and abdomen. The abdomen is easily seen to be made up of a series of annular segments, and a study of its development, and of its relation to allied animals, shows that the thorax and head are constructed on the same type, though in consequence of developmental changes bringing about the fusion of originally separate parts and the suppression of others, it is very difficult to make this out in the head.

The abdomen is devoid of appendages, except in the female, an organ (ovipositor) for the deposition of its eggs; but each of the three segments of the thorax bears a pair of enormously long legs. The middle segment of the thorax also carries a pair of membraneous wings, and the third segment a pair of filamentous bodies with knobbed extremities (halteres or balancers) of very doubtful utility; and believed by some to be the abortive representatives of the second pair of wings possessed by most insects. The appendages of the head must be left for more detailed consideration presently. The integument of the body and appendages is com-

posed of a substance called chitin, resembling horn in its physical properties and forming a resisting outer case or exoskeleton, but hard internal parts are absent.

The internal organs of Tipula comprise a heart in the shape of a long contractile tube on the back or dorsal side of the body, an alimentary canal with accessory salivary and hepatic glands, and a double chain of nervous ganglia placed on the lower or ventral side of the body, with one large ganglion or assemblage of ganglia in the head above the œsophagus, and answering in function to the brain of higher animals. Respiration is carried out by branched tubes (tracheæ) which carry air all over the body and limbs, and whose main branches communicate with a pair of openings (spiracles) in each segment of the body, except those of the head whose tracheæ spring from spiracles at the sides of the neck. The collapse of the tracheæ by accidental pressure is prevented by a spiral thickening of their chitinous lining.

The head of Tipula is elongated in a vertical direction, as shown in the plate, which represents a front presentation as it is seen in the accompanying preparation, but it must be pointed out that the antennæ and maxillary palpi are necessarily constrained by the cover glass to assume unnatural positions. In life the antennæ would be extended in the line of sight towards the observer—that is at right angles to the plane of the paper, and the maxillary palpi would be carried backwards under the head. The first objects to claim attention are the eyes. Examined under a 1-inch objective in a strong reflected light their surfaces will present a hexagonal (sometimes square) areolation, like those facetted lenses or spy glasses which in our childhood days amused and puzzled us with their multifold images. The further study of the insect eye must be made by means of vertical sections. It will then be seen that each hexagonal area or corneule, is a doubly convex transparent body, which must therefore act as a lens, and there is reason to believe that, in some insects, at all events, each corneule is composed of two halves of different density, whereby the production of false colour may be avoided. Behind the corneule, and separated from it by a ring of pigment, which answers the purpose of an iris, in limiting the path of the rays to the central portion, is a transparent cone with convex ends, placed with its base outwards or next the iris and its apex in connection with a single fibre of the optic nerve. All these structures go to make up a single eyelet or ocellite, and each is separated from its neighbour by a layer of dark pigment. The whole assemblage of a thousand or so ocellites, arranged side by side, with their apices converging to one point and their bases forming the external corneal surface, go to make up the single compound eye. The hexagonal form is the result of mutual pressure. Now, though it has been proved that

each ocellite is capable of forming an image at the point of junction with the optic nerve, it does not at all follow that each is the analogue of the vertebrate eye, and that there are formed as many complete images of its surroundings as there are facets in the eye; for in consequence of the pigment separating cone from cone, each single nerve fibre can be impressed only by the light received through the single ocellite with which it is connected, and as the field of view of each is very small, and they are all turned in different directions, a different view is presented to each, though there would be some such overlap as in a pair of stereoscopic photographs. And then who could credit a single nerve fibre with the fearful complex function of transmitting the impression of a complete image? We must therefore conclude that but a single picture is formed by the eye; that picture may be regarded as a mosaic composed of as many points or pieces as there are corneules in the eye. It must necessarily be imperfect, not only by reason of the limited number of points of which the image is composed, but because each nerve-ending is impressed not only with the light from a single point, but from all points—and there may be many—included in its field of view, however small that may be.

The next organs to be noticed are the antennæ. Each will be seen to consist of a large basal joint, followed by a short cup-shaped piece, and then a series of larger but gradually decreasing joints, the whole organ acquiring a beautiful form by the whorl of stiff, bristle-like hairs around the base of each joint. It appears certain that the antennæ are tactile organs, and that they serve in some way for intercommunication between individuals, and there appear reasons for believing that in some insects they have also an auditory function. Below the antennæ, and even exceeding them in length, will be seen a pair of filaments, each consisting of four joints; the basal joint springs from the back of the face, and the terminal one enormously exceeds the others in length, and all are plentifully besprinkled with short, stiff hairs. These are the maxillary palpi. They probably perform a tactile function.

The mouth is situated below and behind, and from it springs a short fleshy trunk, tumid and bilobed at its free end. This organ is the so-called tongue or proboscis. In the accompanying preparation its lobes, which are covered with bristly hairs, will be seen projecting below the face, and in the interior of each will be seen two large tubes with numerous branches; their chitinous lining is thickened in a manner that will at once recall the idea of tracheæ, which these tubes, as a matter of fact, really are, though they have been appropriated to an entirely different purpose from that of respiration. They are channels through which the liquid food of the fly is sucked up into the mouth.

We shall arrive at a more correct understanding of the nature of

the parts of the mouth (trophi), which we have examined in Tipula, if we examine the corresponding parts in an insect in which they are more normal. Let us take the common cockroach. Here the mouth is bounded before by a median horny plate, the labrum or upper lip, and behind by another median plate, the labium, or lower lip, with soft foliaceous appendages, the labial palpi. At the sides of the mouth are two pairs of moveable jaws, an anterior pair of strong toothed cutting plates, without appendages—mandibles, and a posterior pair more foliaceous, the maxillæ, each provided with a large external palp. The trophi, therefore, consist of a median labrum, a pair of mandibles, a pair of maxillæ, and finally a labium, which really represents a second pair of maxillæ, united by their basal parts. In Tipula the labrum and mandibles are represented by three minute pointed styles or setæ contained in a groove on the upper side of the proboscis, and invisible without dissection. The first pair of maxillæ are represented by their palpi which have undergone no abortion, and the labium or confluent second pair of maxillæ, forms the fleshy proboscis.

SPONGE.

THE soft, tough, porous material which lends its name to this article is perfectly familiar to everyone, and most persons have a vague idea that it is a production of the animal or vegetable kingdom, the general impression being that it is due to an insect similar to the creature credited by popular fancy with the formation of coral. Indeed it is really wonderful how comprehensive the term "insect" is in the mind of the non-biological public. Just as any physical phenomenon which is not understood is unhesitatingly ascribed to electricity, so any animal smaller than a mouse, unless it be a fish, is an insect. But even this idea of the origin of sponges, vague and erroneous as it is, marks an advance on a previous state, for it is not so very long since even naturalists of eminence, who had studied the sponge, were unable to agree as to its animal nature at all, many stoutly maintaining for it a vegetable origin. So for fifty years the sponge remained in purgatory unable to find a permanent place in either kingdom. Nor can the biologists of the present generation plume themselves very highly on their comprehension of the nature and affinities of this animal or colony of animals, which still remains in this year of grace 1884 as great an "anomaly" as "the Conservative working man."

In taking up the study of this "form of life," we must be prepared to descend very low indeed in the scale of life—almost to the very

bottom of the animal kingdom—to those realms where heads, limbs, bones, muscles, mouths, vessels, nerves, lungs, and organs of sense, or of anything else for that matter, are left far behind, and the animal or zooid, if it possesses what may without flattery be called a *shape*, may be congratulated on its superiority over some of its near relations.

The material known in commerce as sponge is obtained by divers from the sea-bottom in the neighbourhood of the Greek Archipelago, the Bahamas, and other parts of the world, but it is not the whole animal but only a supporting skeleton from which all the living matter has been removed by washing, squeezing, and bleaching in the sun. If a piece of this sponge be examined with the naked eye there will be seen numerous large, more or less circular openings leading into canals which branch and penetrate the sponge in all parts and freely communicate with one another. A simple lens of high power will show that the substance of this skeleton is composed of an open feltwork of curling and branching fibres of a horny substance called *keratode* (Gr. *keras*, horn; *eidos*, form). In a living sponge this cannot be made out, for the skeleton is then covered with a slimy material and only the larger openings are then visible. Sections of the living sponge in any direction would show that this same slimy substance pervaded the whole interior, covering all the fibres and lamellæ and leaving only a series of narrow, branched canals, the smaller branches of which communicated with a number of microscopic openings called "pores" in the outside of this gelatinous mass.

If a healthy sponge be examined in some of its native water, to which some finely-divided solid substance—say carmine—has been added, it may be observed that currents of water are constantly flowing into these pores, while other currents are streaming away from the larger apertures, called *oscula* (Lat. dim. of *os*, mouth). It is thus evident that there is a constant circulation of water entering the sponge by the pores or *inhalent* apertures, traversing the various channels in the substance of the sponge, and emerging from the oscula or *exhalent* apertures.

Sections of fresh sponge display the fact that the slimy substance —the so-called *sponge-flesh*—consists of an assemblage of nucleated corpuscles or *sarcoids*, about $\frac{1}{2000}$ or $\frac{1}{3000}$ of an inch in diameter, and of irregular and inconstant form. Each consists of a speck of colourless protoplasm, the semi-fluid granular interior of which— the *endosarc* (Gr. *endon*, within; *sarx*, flesh) passes into a firmer clear outer layer—the *ectosarc* (Gr. *ektos*, outside). In the endosarc, besides the nucleus there is sometimes a little cavity—*contractile vesicle*—endowed with the power of rhythmic dilation and contraction. The sarcoid has the power of changing its form by the protrusion of blunt processes—from any part of its body, and

when free from the mass it can crawl about by the same means. It bears, in fact, a remarkably close resemblance to an Amœba. In various parts of the canals, especially near the surface of the sponge, there are found round or oval chambers, lined with a layer of sarcoids, which present some advance in structure on the simple amœbiform type. Their form is usually columnar or oval, they possess nuclei and contractile vesicles, and their outer layer frequently assumes the character of a distinct limiting membrane. Each possesses a long *flagellum* (Lat. for whip) which it is capable of lashing backwards and forwards. In many forms the limiting membrane is raised up round the base of the flagellum into a membraneous collar, and the sarcoid very closely resembles certain collared infusoria. Occasionally very peculiar forms are met with, where the flagellum and collar are borne at the end of a long neck. We can now understand that the currents which transverse the sponge are caused by the co-ordinated lashing in one direction of the flagella in these *ciliated chambers*, as they are called. But how is this co-ordination brought about? We do not know. It is one of the mysteries of protozoic life. Generally on the outside of the sponge, and less constantly in various parts of the interior, masses of nucleated protoplasm occur, which present a variation from the amoebal type in the opposite direction to that taken by the flagellated sarcoids, which we have seen is one of elaboration and specialisation. The masses in question present a degradation of structure, for the sarcoids of which they originally consisted have lost all their individuality, and fused into a continuous film, or *syncytium*, as Haeckel calls it, and all that remains to mark their presence is their nuclei, but the mass still retains its functional activity.

In some very few sponges (Myxospongiæ) there is no skeleton. In the others the skeleton is usually strengthened, and, in some cases, entirely formed, by spiculæ of carbonate of lime or silica. These spiculæ are of most varied and beautiful forms; if the reader wishes to see what a great variety there is, he must be referred to Bowerbank's splendid monograph, published by the Ray Society, in which he will see two or three hundred distinct forms beautifully figured, and if he has nothing else to keep him out of mischief, he may occupy himself by learning by heart the two or three hundred names invented for their designation. Just to incite him to this study we quote three of these names—Exflected elongo-equiangulated triradiate, Furcated attenuato-patento-ternate, Torqueato-tridentate inequi-anchorate. We confess with all humility that we have not these terms at our fingers' ends. It is evident that these spiculæ must tend to the preservation of the species possessing them, for by rendering them altogether unsuitable for the purpose of man, they are protected from his depredations, and

they must enjoy an equal immunity from the attacks of other creatures who would otherwise prey upon them, for a mouthful of the spicules would by most animals be relished about as much as would a mouthful of their descriptive terms by the average mortal. In one order of sponges—the Calcispongiæ—the skeleton consists entirely of interlaced spiculæ of calcic carbonate. In another order—the Fibrospongiæ, a fibrous keratode skeleton is always present, and in most forms siliceous spiculæ are abundantly distributed, not only through the keratode, but in the sponge flesh, Halichondria, a section of which is issued with this number, belongs to this order, and shows well the fasciculi of acicular spicules. The sponges of commerce are those forms of horny sponges which contain no spiculæ. In other sponges, such as the beautiful Euplectella, the spicules are very abundant and very closely united, and the quantity of keratode is excessively small. Finally, in Clionia there is no trace of keratode, and the skeleton is entirely siliceous.

The reproduction of sponges is effected by a sexual process. Nucleated cells, exactly resembling the *ova* of higher animals, are formed by certain of the sarcoids becoming detached, and acquiring a spherical form while retaining their nuclei and nucleoli, while certain other sarcoids undergo changes resulting in the breaking up of their contents into numerous minute bodies—*spermatozoa*, provided with long vibratile filaments, by which they can propel themselves through the water. Fertilization is brought about by the contact of one or more spermatozoa with the ovum, the interior of which then breaks up into two portions which sub-divide again and again, until the originally single cell comes to consist of a hollow oval chamber whose walls are composed of two layers of cells—an inner—*endoderm*, and an outer—*ectoderm*. At this stage the embryo is free, and swims about by means of cilia with which the ectoderm is covered. One end of the embryo then turns in and converts it into a hollow sac, and the *gastrula*, as it is now called, attaches itself by the closed end to some object at the sea bottom, and loses the cilia of its ectoderm, the cells of which unite closely with each other to form the *syncytium*. Pores appear here and there in the syncytium, through which inhalent currents are caused to set in by the cilia of the endoderm, and the water is discharged by the opening at the apex, which forms the single exhalent aperture or osculum. Before this period, however, spicules have made their appearance in the ectoderm, and young sponge has acquired a tolerably complete skeleton. Many of the Calcispongiæ remain permanently in this condition of a hollow chamber with thin walls and a single osculum. In the more complex sponges further development takes place mainly by the growth of the syncytium, whereby the endodermal cells become separated

into small groups, which are ultimately restricted to the ciliated chambers.

In Spongilla, the only sponge inhabiting fresh water, an asexual process of reproduction also occurs. Certain of the amœbiform sarcoids retract their processes, become surrounded except at one point, by a spiculigerous wall, and after a period of rest are set free in the water and each reproduces the parent form.

The affinities of the sponges are very imperfectly understood, and they are among the most difficult animals to assign to their proper place in any system of classification. The resemblance of the sarcoids to Amœbæ and Flagellate Infusoria is so close, that from a study of the mature forms they would be unhesitatingly placed in the lowest sub-kingdom—the Protozoa. But among the Protozoa sexual reproduction is very rare, and a segmented ovum with the subsequent formation of a multicellular gastrula is a thing altogether unknown, while this character unites the sponges with the members of the next higher sub-kingdom—the Cœlenterata, from which, however, they differ very widely in their subsequent development.—*Cole's Studies.*

STARCH.

From Cole's Studies.

WHEN exposed to sunshine, all green parts of healthy living plants, and especially the leaves, are the seat of remarkable chemical activity. The air, with its small but all-sufficient quantity of carbon dioxide, which has gained access to the green cells by means of the stomata and the intercellular passages, and the water forming the chief constituent of the cell sap, being brought into close relation to each other there result a series of most important synthetical operations which cannot be brought about by any other means known to chemists. Under the influence of the sunshine and the chlorophyll the carbon dioxide ($C\,O_2$) is decomposed, its oxygen is liberated, and the carbon and the water unite with each other directly, in certain proportions, to form organic compounds of greater or less complexity, which are used by the plant for the growth of the young cells, and the nutrition of all living parts. Among these first products of *assimilation*,* as this process

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* This term is here used in the perverted sense customary among botanists. Properly assimilation is the process by which the prepared nutriment is incorporated with the actual substance of the plant, and becomes part of its body.

is called, are sugar, oil and fat, and *starch*. When assimilation is in excess of the present demands of the plant, the excess of nutrient matter is removed from the cells in which it is formed, and stored up in other parts for future use. When the first-formed products are soluble in the cell sap, their transmission is of course readily explained by the constant circulation of sap, but when insoluble the transmission must be otherwise accounted for. The substance, in fact, must undergo a process of *metastasis*, or conversion into some soluble body, and this, on its arrival at the place of deposition, may be reconverted into its original insoluble state, and so remain until it is required for use.

Of these substances *starch* is by far the most important. It is one of a class of bodies called carbo-hydrates, that is, it consists of a compound of carbon with oxygen and hydrogen in the proportion of 1 atom of oxygen to 2 of hydrogen, or in other words, in the proportion in which they exist in water. So we may regard starch as a compound of carbon and water. This fact should be borne in mind in connection with its use as a dietetic, for its hydrogen being already completely oxidised or burnt cannot contribute to the production of heat in the body, and so does not take equal rank as a heat producer with the fats and oils in which the hydrogen as well as the carbon is in excess. Starch is insoluble in water and cell sap. When analysed it yields the proportional constitution $C_6H_{10}O_5$, but these figures do not represent the true complexity of its molecule, which is best represented by $nC_6H_{10}O_5$, in which n is used in the algebraical sense to stand for an unknown quantity, but probably $= 3$, so that the true formula would be $C_{18}H_{30}O_{15}$. It is closely related to *glycogen* or liver starch, *dextrin* and *cellulose*, and although all these bodies differ in their physical and many of their chemical properties, they have exactly the same composition, or, as chemists say—they are *isomeric*. From glucose or grape sugar, the formula of which is $nC_6H_{12}O_6$. It differs only in the possession of one molecule less of water, and is easily convertible into that substance. Treated with a very dilute solution of *iodine* starch assumes a purplish blue colour, and this reaction being very delicate and highly characteristic of starch, affords the most valuable test for that substance, even the optical one to be presently mentioned not excepted. It has been usually assumed that the iodine formed a definite compound with the starch (the so-called iodide of starch), but there is reason to believe that it is only deposited on the starch in a metallic state.

By the action of an organic nitrogenous body of very doubtful composition, called *diastase*, produced in germinating seeds, and by other means, starch is converted into dextrin and glucose, both of which are readily soluble in water or the cell sap. This, of course, is important in explaining how the insoluble starch may

find its way from the chlorophyll grains, in which it originates, to the cells where it is stored.

Starch is first formed in the interior of the chlorophyll grains as minute, rounded, solid particles. During the whole time the green leaves are exposed to sun light, an accumulation of these particles occurs in the chlorophyll grains, but as soon as the light fades the quantity which has been accumulating all day decreases, and the starch is gradually removed in the form of dextrin, or some other soluble form, then on reaching the receptacle in which it is to be stored an inverse change ensues and the dextrin is once more converted into starch, in which form it is deposited. Although the process of removal of the original particles of starch from the chlorophyll grains can be traced only at night, it, in all probability, goes on constantly, but during the daytime the rate of removal is insufficient to counterbalance the larger amount of new material formed.

In the chlorophyll grains the particles of starch never reach any higher degree of organisation than that mentioned above, but in the stems, tubers, roots, rhizomes, seeds, and other parts where it is stored for future use, it assumes the form of complex and definitely-organised grains, whose form is characteristic of the genus or species in which they occur. The grains are frequently of a large size, but the size varies considerably, being in some plants almost immeasurably minute, in others as the Potato and Tous-les-mois, attaining a diameter of as much as a four-hundredth of an inch, and being readily visible with a simple lens. In the same plant, and even the same cell, the size varies considerably, being dependent chiefly on the relative age of the grains, so that when measures of starch grains from various sources are given, they must be taken only to represent an average, and much latitude must be allowed for individual variation. In some plants, though —the Sarsaparilla for instance—the variation in size is less marked. The forms of the starch grains are as variable as their sizes. In the potato they are oval, in the bean elliptical, in some orchids spherical, in the wheat grain lenticular, in the maize polyangular, in ginger root, like short bent rods, and in the laticiferous cells of Euphorbia peculiar bone-like forms occur. In the oat the grains are compound, consisting of a number of closely packed, but readily separable, granules.

Examined under a sufficiently high power, and in a suitable medium (50 per cent. glycerine answers well), a dark spot will be seen in most grains. This is called the hilum or nucleus and is usually placed eccentrically. In those grains which are elliptical, it is placed nearest the narrow end of the grain. Surrounding the hilum will be observed a number of zones, alternately light and dark, due to alternations of more and less watery layers, and besides these alternations of much and little water in the layers,

there is a progressive increase in the quantity of water or decrease in the density of the layers from without inwards, the hilum being always the darkest and most watery part. That this is a correct explanation of the appearance of the layers may be seen by observing the grains in a medium—alcohol, for instance—which abstracts the water, and entirely, or almost entirely, obliterates the appearance of zoning. When the grains are allowed to dry, the striation is also very indistinct, and the place of the hilum is then usually seen to be occupied by a cavity containing air. A stratum of air also sometimes occurs between two layers, and brings them into view when they would otherwise be invisible. In the dry grains, too, a number of cracks may be seen radiating from the hilum, and produced by the greater shrinkage of the central parts of the grain in consequence of the greater loss of water from them than from the outer layers.

Concerning the mode of growth of the grain, and the origin of the layers, there has been, and is, much dispute. One set of observers state that the growth takes place as in a crystal, by accretion or deposition of layers, alternately more or less hydrated, on the outside of the grain, so that the outermost layers are the youngest. But Sachs, following Naegeli, maintains that the grains grow by intussusception or deposition of molecules in the interior of the grain between already existing molecules, and weighty arguments are adduced in favour of this view.

The excentric position of the hilum is thus explained. The starch is never deposited in entirely dead and empty cells, but usually in cells whose vital activity is reduced, and but a lining zone of protoplasm remains. It is in this layer that the growth of the grain commences, and as it increases in size it is pushed towards the centre of the cell and is no longer entirely surrounded by protoplasm, and the part so removed from contact with the protoplasm naturally grows more slowly than the side that is still imbedded. Sometimes the starch is deposited in hollow vesicles in the protoplasm, then as growth takes place the grain extends into the cavity away from the protoplasm, and the same irregular growth as before results.

The hilum commonly conforms to the shape of the grain. In elliptical grains it is elongated in direction of the longest axis of the ellipse, and in lenticular grains the hilum is lenticular. Occasionally, two or more nuclei appear in one grain, and concentric layers are deposited around each. This is often seen in the haricot bean. When it occurs the most rapid growth usually takes place in the line joining the two nuclei, and a rupture at length takes place, whereby the original single grain becomes divided into two, though they may still remain in contact with each other. Such compound grains occur in Sarsaparilla. Examined

under a quarter or eighth inch object glass, the cells of the cortex and medulla will be seen to be filled with rounded grains, most of which show traces of a division into three separate granules. In the balsam preparation it is not easy to see the striation for the reason above given, but some grains are sure to shew it under a suitable illumination. It will then be seen that the layers do not encircle the grain as a whole, but each granule has its own hilum surrounded by its own concentric layers. Very often one or two divisions are much more pronounced than the rest.

Starch assumes a most characteristic appearance under polarised light. Space will not allow, nor is it needful to enter into an account of the nature of polarised light, with which we must assume our readers to be in some degree familiar. It will suffice here to draw attention to the phenomena presented by its use. When starch is examined under crossed Nicols the field remains dark, but each granule assumes a glistening grey appearance, as if self-luminous, and is marked with a black cross. If, then, the object be slowly rotated in the field of view, it will be seen that the cross remains fixed with regard to the field, one pair of its arms being parallel to the principal plane of the polariser and the other parallel to the principal plane of the analyser. As the arms of the cross, however, are frequently curved, their direction does not always appear to coincide with these planes. During the rotation, in fact, the grain appears to be turned round underneath the stationary cross.

If, the object remaining stationary, the polariser or the analyser be rotated, the cross will be seen to rotate with it but with only half its angular velocity, so that to make a complete rotation of the cross the analyser or polariser must be rotated twice. If now a thin film of selenite be interposed between the polariser and the object while the nicols are crossed, and be rotated until it gives the brightest field, most beautiful chromatic effects will be obtained. The field will assume a colour dependent upon the thickness of the selenite film, and the interference crosses will be vividly coloured, the rest of the grains assuming a complementary colour. For instance if a yellow-blue selenite be employed and be so adjusted in the first instance as to give a blue field the crosses will be red at the edges merging into yellow in the centre, and the interspaces will be bright green. Then on rotating the analyser or polariser, as the blue field gradually merges into the complementary yellow, so the crosses rotate and change to their complementary colours. By means of the interference phenomena under polarised light, the compound nature of the grains in sarsaparilla is most clearly shewn, for each granule exhibits its own cross and its own chromatic effects. In studying this preparation, the medulla or pith is the most favourable part, for here the cells are largest and

the starch less closely packed. Some grains will be met with that are not compound and exhibit but a single cross, others will be presented under different aspects, some showing the triradiate division of the granules, others a single diametral suture when the grain is seen from the side. Frequently the normal spherical form is departed from. Round the edges of the cells especially, the grains have, by mutual pressure, assumed the form of very short truncated cones with rounded angles. If the observer searches the cells carefully he will probably be rewarded by finding a few isolated granules, the appearance of which will of course vary according to their presentment, and these will afford a better idea of the true form of the granules than could possibly be obtained by any other means. Occasionally grains are met with having more than three component granules.

The South American genus Smilax, from the roots of several species of which the sarsaparilla of the Pharmacopœia is obtained, belongs to the natural order Smilaceæ, which together with a few other orders, presents a remarkable departure from the normal type of monocotyledons. The form of the embryo and of the flower, the minute structure of the ærial stem and its branches, and the general character of the plant are exactly those of other monocotyledons, but the veins in the leaves form a network and the *rhizome* or creeping underground stem has its woody tissue disposed in a ring round a central pith or *medulla*, and is surrounded in turn by a parenchymatous cortical layer. These characters are as distinctly dicotyledonous as those before mentioned are monocotyledonous.* The arrangement of the bast and xylem in the root is somewhat different, for instead of each bundle consisting of an internal woody portion separated by a cambium ring from an external bast portion the two constituents are arranged collaterally, large xylem bundles consisting of large vessels and thick-walled prosenchymatous cells alternating with much smaller bast bundles composed in the main of sieve tubes, the whole being surrounded by a single layer of very thick-walled cells, representing the vascular bundle sheath. Under a 1 inch objective this arrangement will be well seen in the accompanying preparation, and a $\frac{1}{4}$ inch applied to the sieve tubes will show here and there the perforated end walls or sieve plates between the ends of adjacent vessels. All the elements of the root are highly lignified, and polarise strongly without the aid of a selenite.

* Contrast this with the trans. sect. of the Bulrush, which is an ordinary monocotyledon.

NOTES AND QUERIES.

NOTICE.—As this Journal will be suspended for some time, after the issue of the December Number and Index, Subscribers are requested to pay their outstanding Subscriptions at their earliest convenience to Messrs. Brook and Chrystal, 11, Market-street, Manchester.

FLOSCULARIA MUTABILIS.—This organism which Mr. Bolton proposes to name as above, on account of its curious habit of varying its trochal disc, has been sent out from Newhall-street during the past month. All the specimens hitherto found have been free-swimming. They resemble *F. campanularia*, but have only two lobes instead of five. There is a pair of eyespots on each side of the longer lobe. Besides the stiff or radiating cilia, characteristic of the Floscules, it has in addition a wreath of active vibratile cilia round the trochal disc. It possesses a gelatinous sheath in which the eggs may often be seen.

PROF. ATTFIELD'S PAPER.—Some of our readers may think, perhaps, we have strained a point in admitting this paper into *The Microscopical News*. We have, however, thought the subject to be of such interest and importance, that we hope some of our readers will take up the debatable matter from a microscopic point of view.

THE CELL WALLS OF DIATOMS.—A very interesting paper on this subject appeared in the August number of the Journal of the Royal Microscopical Society. The "methods of investigation" deserve careful study.

BELGIAN DIATOMS.—Dr. H. van Heurck has published the first two sets of slides, illustrating his Synopsis of Belgian Diatoms. The specimens (fifty species) are for the most part preserved in a mixture of styrax and liquidambar.

DANGERS FROM THE EXCREMENTS OF FLIES.—Dr. Grassi has found that the eggs of human nematode parasites are swallowed by flies and afterwards deposited in the excrement. It would be interesting if some of our English observers would follow up these observations.

LA TRICHINE ET LA TRICHINOSE.—An octavo treatise of 257 pages and 15 plates, published last year in Paris as a Government work, by M. J. Chatin, and which should be read by all who are engaged in the prevention of the spread of disease by means of our food supplies.

STYRAX AND LIQUIDAMBAR can be obtained purified, especially for mounting microscopic objects, from M. M. Rousseau, 42-44, Rue des Ecoles, Paris.

THE BOLTON MICROSCOPICAL SOCIETY.—Since the Summer recess the Society has had two meetings, on each of which interesting papers have been given. At the September meeting Mr. W. Tyson gave a very interesting account of various forms of starch and the method of preparation, after which he gave various samples to the members present, in order that they might examine them at leisure. At the meeting held in October, Mr. Parr, Hon. Sec. of the Bury Natural History Society, gave a well-illustrated paper on the Mouths of Insects. After the Lecture various slides illustrative of the paper were examined.

LIVERPOOL MICROSCOPICAL SOCIETY.—The seventh ordinary meeting of this Society was held on Friday, October 3, when Mr. Frank T. Paul, F.R.C.S., read a paper on "Some Experiments with Microphotography," illustrated by the oxyhydrogen lantern. After the paper, the following objects were exhibited :—

Crystals of Sugar (Cane) Polarised; Eyes of various Insects, living; Fern fructification in situ, varieties; Fibro cells of Oncidium; Fruit of Bidenscernua; Lunar Photograph, "Copernicus"; Pistil of Arbutilon; Section of Eye of Drone Fly; Seeds, various; Specimens illustrative of the Paper; Spiracle of Dytiscus; Spores of Equisetum; Tran. Section Pteris Aquilina, Polarized.

DR. KOCH AND THE CHOLERA BACILLUS.—It is reported from Berlin that Dr. Koch has succeeded in communicating cholera to a number of rabbits by inoculating them with pure cultures of the "comma" bacillus. The rabbits at any rate sickened and died with symptoms resembling those of cholera. The intestines were found to be infested with the "comma" bacilli. Should this announcement be confirmed it will go a long way towards establishing Koch's opinion that the microbes in question constitute the true virus of cholera. Nicati and Ritsch are said to have anticipated this experiment in Marseilles about three weeks ago, and to have obtained the same results.

THE CHESTER SOCIETY OF NATURAL SCIENCE.—A very nice little handbook has been issued under the auspices of this Society. It is entitled "A Short Handbook of Natural History," for use at Annual Conversazione and other meetings of the Society. It is published at the modest price of sixpence, and may be obtained from Mr. G. R. Griffith, Grosvenor-street, Chester.

THE MICROSCOPICAL NEWS

AND

NORTHERN MICROSCOPIST.

No. 48. *DECEMBER.* 1884.

THE USE OF POLARIZED LIGHT IN VEGETABLE HISTOLOGY.

Translated from the German of Professor Dr. L. Dippel. *Zeitschrift für wissenschaftliche Mikroskopie*, 1884.

By W. Blackburn, F.R.M.S.

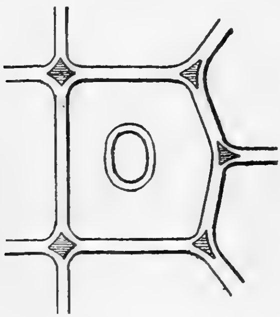
OBSERVATION with polarized light in vegetable histology has hitherto not found very extensive employment, except in a few instances, although it is of much importance to the decision of many questions. The reason of this may depend partly upon the fact that it is still by no means certain how the phenomena arise, which present themselves by this method of observation in the vegetable cell, especially in the cell-wall, as well as in certain parts of the contents; whether they arise through molecular change in the building-up of the organic substance or through tension-relations. On the other hand, the reason may be that certain relations of structure, &c., (which observation by polarized light is able to explain) may also be ascertained by other methods, as, for instance, by chemical re-actions, colouring, &c. One reason may also be the fact that if one would aim at results which admit of no doubt, this method of observation requires the most successful preparations in all cases where fine structure-relations are concerned, whilst in incompetent preparations there is little or nothing to be seen. Whereas, upon the one hand, observation with polarized light affords abundant proof of results gained by other methods, and, upon the other hand, may be able to throw light upon facts previously observed, which otherwise would not be so completely confirmed as by its means, therefore this means should nowhere escape notice, when the conditions of the case point to its application.

These lines should serve to direct deserved attention to a method of observation which has afforded me much assistance in my researches into the finer structure of the cell-wall; and I will here briefly

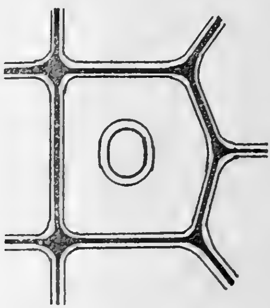
discuss some facts, to the confirmation of which it affords the most valuable aid.

If we examine in common light a very thin transverse section, cut quite perpendicularly to its long axis, through a tissue with thickened cell-walls, *e. g.*, through the wood of a leafy or coniferous tree, we see, as a rule, the well-known net-work (which I designate the net-work of the primary walls, whilst Hofmeister, Sachs, and others have conferred upon it the name of "middle-layer") in a manner which causes us to conceive the same as a structural form common to contiguous cells, in which, beyond the well-known gusset in the angles, where three or four cells are joined together, no further differentiation is perceptible. (Fig. 1.)

We now examine the same transverse section by means of polarized light, and in sooth in the dark field of view (with the crossed Nichols) appears a substantial alteration of the appearance



1.



2.

of the so-called middle layer. The whole of the previously homogeneous net-work appears, whilst the primary partitions shine in clear white light, (about what happens to the other parts of the wall nothing need here be said), with a fine black line drawn through them from the gussets, thus forming three stripes, of which one of those illuminated belongs to one of the neighbouring cells, the one not illuminated appears common to both. (Fig. 2.) Observation with polarized light thus tells us very decisively that the middle layer is not single, but consists of three plates, the middle one of which is singly refracted, whilst those on each side are doubly refracted, the one thus indicating a totally different molecular structure, and even chemical constitution, from the other two.

It is true that there are other means of recognising the constitution of the middle layer; but how easily is one disposed, by a little

prejudice, to give another interpretation to appearances upon which facts are founded, or to exaggerate the significance of facts established by inexact observation.

That primary walls, each belonging to two different cells, enter into the formation of the middle layer, we recognise at once in cases where, in the natural object, the middle plate becomes loosened through any circumstances (as in the wood of some old conifers); and this is further demonstrated by the results of maceration. But has it not been attempted to explain the separation in the former case as the effect of tension-relations; and, in the case of maceration, has not the decay of the tissues been explained by the loosening of the entire middle layer, composed, as it is, of the three plates?

Furthermore, it can be proved beyond doubt that the middle plate, in its chemical behaviour, differs from the cell-walls; indeed the usual re-agents upon the cell-substance prove this, as well as tinting, only, however, after suitable preparation, as I have explained in another place. But has it not been tried to depreciate these facts by a few re-actions, often executed by very juvenile hands, mostly only after tormentings of the preparations concerned in different ways, by means of the most various expedients, the effect of which on the molecular relations of the cell-walls is beyond control, and by means of which, in one case, a transitory, feeble, and in another a dubious blue colouring has been claimed to have been revealed, and thus to disprove the assertion that the middle plate could not have formed a strong woody part of the original wall, consisting of cell-substance?

On the contrary, observation with polarized light, in both cases, leads to unassailable results, provided, however, that the specimens have been successfully prepared. In the former case, such observation shows, in all stages of the influence of the macerating medium, that only the middle plate (first made known through polarization) is subject to loosening; whilst in other cases it proves that the lignification of the substance of the cell-walls neither lessens nor suspends the double refraction, seeing that the same (the middle plate) through all stages of preliminary treatment of the section, not subjected to re-agents, to the complete removal of the woody substance in the primary walls, retains the same degree of strength as in the condensation-layers. At the same time it proves with absolute certainty that the middle plate, recognised as singly refracted at the first observation, remains so until it arrives at the point which precedes the complete separation, and when it is no further dyed yellow by iodo-chloride of zinc, and when the cell-substance, if it existed, must thus be recognised, if only feebly, by double refraction.

If then the fact that the middle plate of the "middle layer," in

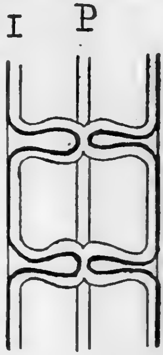
the complete state, appears under all circumstances as a part of the singly refracted cell-wall proves that it is different in its molecular structure from the primary walls, as it is from the condensation-layers, and contains no cell-substance even in its final condition, still the question remains to be decided (for the middle plate must be considered, in consequence of its position outside the primary walls, as a constituent part of the cell-wall, formed before the primary wall) of what kind is the first solid secretion-product of the living cell-body, *viz.*, the first wall-formation, to which it (the middle plate) owes its origin? Here also observation with polarized light again affords an indubitable explanation, if we apply the same to the cell-wall during its development.

A transverse section through the cambium-region (of a conifer for the best), when observed in the darkened field of view, shows the following state of affairs :—

Varying according to the season of slow or more rapid cell-multiplication, in which the wood was formed, the walls of a single cambium-mother-cell, or of several rows of cells, proceeding from the wood to the bast (the first generation of daughter-cells), appear darker than the ground of the field of view, whilst the same condition is recognised in the procession of those rows of cells lying next to the wood and bast, from those with still proportionally thinner walls to those with walls already more or less thickened, as aforesaid. Thus it is possible closely to follow the transit of the dark stripes through the bright net-work of the primary walls with the dark walls of the cambium-cells. Now these facts prove that, prior to the primary wall being formed out of cell-substance, and manifesting itself at once by its double refraction, an envelope, singly refracted, and therefore not formed out of cell-substance, is secreted from the protoplasm for every daughter-cell originating in a cambium-mother-cell, which envelope remains during the conversion of the cambium daughter-cells into wood or bast-cells, and thus becomes the middle plate of the middle layer. The matter is therefore decided, and a firm ground obtained for the interpretation of the appearances which we obtain by re-agents upon the cell-substance, as well as by means of carmine and aniline dyes, as I have described in another place; and what I there said I must now maintain as correct, on the ground of my extensive and careful research, *viz.*, that the middle layer is compounded of the two primary walls of adjoining cells, and of the cambium-layer common to each.

Concerning the share to be assigned to the wall-layers in the formation of the pore-canals and the closure of the pores, various opinions notoriously prevail. With H. von Mohl, Schleiden, and others, some of the more recent botanists share the view that the closure is formed by the primary walls, and that the entire

secondary walls pass into the pore-canals; while Theodor Hartig advances the opinion that it is the innermost layer of the cell-wall (the inner membrane of the newer, the tertiary membrane of the older authors) which is transformed into pore-canals, and that the closure is formed by two adjoining cells here brought together (Fig. 3), which opinion I can confirm, after carrying out a series of researches. The fact, as represented in the annexed figure, is now also admitted by Strasburger, but another interpretation is given to it. According to this investigator, the inner layer is said to represent a later differentiation, which arises in consequence of contact with the cell-contents; and the stronger light-refracting layer, which only apparently extends from the inner plane uninterruptedly into the pore-canal, is said to represent just such a differentiation of the secondary thickening in the parts adjoining the pore-canal, whilst the closed end of the pore is formed out of the primary walls.



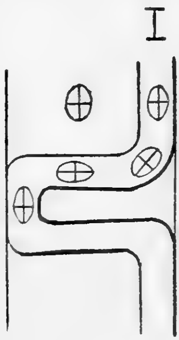
3.

Whereas the collective layers of the cell-wall possess the form of an ellipsoid of elasticity, which, in the transverse and longitudinal sections, appears as a section of an ellipse, in which the smallest axis lies radially or perpendicularly to the stratification, the greater axis parallel to the stratification (the transverse section yielding the least, the longitudinal section the greatest axis), so observation with polarized light must afford the necessary and most trustworthy elucidation of the course of the stratification. If the course of the inner layer is in this way, as Hartig and I maintain, then the arrangement of the section falling through the ellipsoid of elasticity must change in the section-plane, just in the way in which it is represented in the schematic figure 4. If, on the contrary, the real structure is in accordance with the view of Strasburger, then such a change cannot take place, and the arrangement of the ellipsoidal section in the parts in question of the wall must be as represented in the schematic figure 5.

If we now observe in the field of view (coloured red by means of a small selenite plate) a section sufficiently thin (so thin, indeed, that in the supposed arrangement of the experiment, the added colour will rise only to violet, the withdrawn colour fade only to orange) through the seed-albumen of *Phytelephas macrocarpa*, which object is one of the most suitable on account of the deep pore-canals, then the inner layer, like the other wall-layers, will appear in yellow light under an angle of $+45°$ in the place where the inflection of the pore-canal lies; therefore, where the axes of elasticity of the inner layer run parallel with the planes of polariza-

tion the colour of the field of view stands out, and at the same time the pore-canal, which is revealed under -45°, appears in blue; and these colours are inverted if we change the position to 90°, *i.e.*, where yellow was before, blue appears, and *vice versâ*. This fact confirms the change of the ellipsoidal section represented in Fig. 4, and also the uninterrupted course of the inner layer through the entire hollow chamber of the cell, including the pore-canals with the inclosing wall, as it is represented in Fig. 3.

Through chemical influence (to which I will refer in my next work in another place), the inner layer is loosened, and the adjoining closure-wall disappears; thus the inner walls of the dilated



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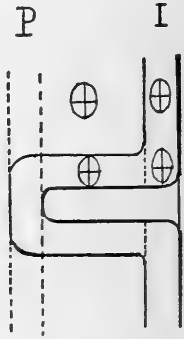

5.

pore-canal, into which now the secondary thickening directly flows, do no longer show the aforesaid colour-difference; on the contrary, all the wall-layers, not having shared in the separation, and having remained nearly as strongly doubly refracted as before, appear coloured yellow or blue, according to their position under + or -45°.

Extremely striking results are afforded by polarized light when spectrally dissected. With the previously considered object (transverse section of *Pinus sylvestris*), in the parts of the transverse section lying above the dark Müller's band (produced in the green of the spectrum, through a small red selenite-plate, with the centre near Fraunhofer's line *b*), the singly refracted cambium-wall appears most distinctly as a dark stripe between the primary walls, shining in the most brilliant green; whilst the other parts, lying over the colour-regions of the spectrum not affected by the selenite-plate (and indeed lying in the regions where the colour of the selenite

plate, through the interposed object in its doubly refracted wall-parts, rises to blue or sinks to yellow, and where the shifting of the Müller's band to red or blue takes place), show the cambium-wall as a stripe, shining in the respective colours, between the strongly darkened, almost black, primary walls.

In the second preparation (longitudinal section through the seed-albumen of *Phytelephas macrocarpa*), we select such a suitable spot that, in the red-coloured field of view, in the usual experimental arrangement for polarized light, the aforesaid colour-phenomena, which indicate the change of the ellipsoid of elasticity, stand out the finest, and we observe at this spot the appearances in the spectrally-dissected polarized light; then we find the most decided and undoubted evidence in the inner layer of the change in the direction of the two effective axes of elasticity. We have arranged the optical part of the spectro-polarizer as suggested in the "Handbuch der allgemeine Mikroskopie," p. 984, whereby the greater axis of elasticity runs parallel to the ends, the smaller parallel to the side of the object-slide; and we now insert the section of the longitudinal wall lying between two neighbouring illuminated cells, over Müller's band, when the collective wall-layers, consisting of cell-substance, whatever course they may take, appear in green. We push this wall towards the red of the spectrum, when the wall-layers appear running together parallel to the long axis (and therefore to the ends of the object-slide), in which layers the greatest axis of elasticity of the flat section is arranged in the same way as the same axis would be by means of the selenite-plate, somewhat obscured in the orange; whilst the inner layer, so far as it reveals the pore-canal, is brightly lighted, and thereby informs us that in this same spot, in which the effect of the axes of elasticity takes place, the less axis is arranged by the selenite in a direction contrary to that of the greater, and therefore assumes a position in the section of the ellipsoid of elasticity approaching to 90° to the part running lengthwise. When the movement is made towards the other end of the spectrum, the appearance is reversed, an obscuration of the revealed inner layer of the pore-canal takes place in the blue, between Fraunhofer's lines F and G, whilst the wall-layers running in the direction of the long axis leave the connected colour-region in unchanged brightness.

Some further observations in polarized and spectrally-dissected light, especially in reference to the change or persistence of the degree of double refraction under various circumstances, and upon the question whether double refraction of the cell-walls, of the starch-granulations, &c., may depend upon tension-relations or upon differences of molecular constitution, shall be reported in another communication, because I am now occupied in appropriate researches.

FUNGUS FORAYS, 1884.

HACKNEY NATURAL HISTORY SOCIETY.—The Foray of this Society was made on Saturday, 27th September, to Epping Forest. Although the general Foray did not commence till after noon, some of the members were on the ground and commenced the search early in the morning. It was expected that the dry season would have its effect in limiting considerably the number of fungi to be found, and this was in reality the case, for long walks had to be taken in order to secure a very limited number of species. Most of the baskets contained only common species, but two interesting additions to the British Flora were determined. One of these was *Hydnum diversidens*, Fr., found by Mr. H. T. Wharton and Mr. J. C. Webb, on a trunk near Fairmead; the other was *Boletus duriesculus*, Kalch., an ally of *Boletus scaber*, and probably may have been confounded with it in times past. After tea at Fairmead Lodge, the specimens were laid out in an ante-room, and examined leisurely by the party, information concerning them being furnished by the President, and Messrs. Worthington Smith, H. T. Wharton, and James English.

ESSEX FIELD CLUB.—Two days having been selected for the Foray this year, the members met at Loughton on Friday, October 3rd, and, accompanied by the Rev. Canon Du Port, Mr. W. Phillips, of Shrewsbury, Mr. Worthington Smith, and M. C. Cooke, proceeded towards Monk's Wood, in Epping Forest, then through other portions of the Forest, reaching Buckhurst Hill in the afternoon, when the specimens were arranged on tables in the large ballroom of the "Roebuck," and duly named, labelled, and classified, Mr. T. Howse having sent *Hydnum erinaceum*, and *Boletus aurantiporus*, and other species, from Guildford. On the following day other portions of the Forest were explored, terminated by a tea at five o'clock, and a meeting thereafter, at which the results of the two days' Foray—as far as they could be ascertained at the time—were reported, and Mr. Worthington Smith read a paper on the "Politics of the Potato-Fungus." Notwithstanding that the season was unfavourable, a good exhibition was made, and a great number of visitors were clustered around the tables until a late hour. The Rev. J. M. Crombie exhibited an excellent collection of the Lichens of Epping Forest, and a large number of microscopes at a central table displayed objects allied to the subject of the day in a most efficient manner. Between 20 and 30 species, not before recorded, were added to the Epping Forest Catalogue.

LEICESTER PHILOSOPHICAL SOCIETY.—The first Fungus Excursion of the Biological Section of this Society was made in Charnwood Forest on Wednesday, October 8th. The morning was by no means promising, and consequently but few members came to the starting post. About noon the rain began a continuous drizzle, which, by 3 p.m., settled into a regular downpour. Foraging had to be conducted for some time under considerable difficulty, and finally abandoned. No rare species were met with, but an accurate list was kept of all that were examined and determined during the day, so that, in the evening, when the results were compared, it was found that some forty species had been added to the list of the Fungi of Leicestershire. The sole lady of the party exhibited some very characteristic sketches which she had made of several species of *Agaricini*, and we then, as now, entreated her to persevere, and, by so doing, perform good service for Leicestershire botany.

THE WOOLHOPE CLUB FORAY.—The usual week at Hereford commenced on October 13th, and the first excursion to Leominster for Croft Ambury on the 14th. The walk was pleasant, the weather and company agreeable, and the view extensive and picturesque; but many of the baskets remained almost empty, most of the time being occupied in marching up a hill and then marching down again. On the 15th a short excursion to Haywood Forest was much more satisfactory in its results. On the 16th the general excursion was to Dinmore, where the beautiful *Cortinarius triumphans* was found, again under birches, the only previously known locality being Haywood Forest. On October 17th, the last, and worst, excursion was made in Eastnor Park, near Ledbury. During the evenings the following papers were read at the soirées: "Notes on the Edible Fungi of North Italy," by A. S. Bicknell; "On Colour Nomenclature in Fungi," by H. T. Wharton, M.A.; "British Species of Nidularia," by W. Phillips; "The Spermogonia of the Uredines," by C. B. Plowright; "Researches into the Oospores of some Fungi," by the Rev. J. E. Vize, M.A.; "On Bunt," by C. B. Plowright; "Recent Views on the Lamellæ of the Agaricini," by the Rev. J. E. Vize, M.A.; "Some Recent Additions to our Mycologic Flora," by W. Phillips, F.L.S.; "Trinomialism in Zoology," by H. T. Wharton, M.A.; and "Some Gigantic Fungi," by M. C. Cooke.

The week was conspicuously deficient in novelties, which were chiefly confined to those sent from a distance. A. O. Walker, Esq., of Chester, sent a box of specimens from North Wales, which contained nothing rare. H. T. Wharton exhibited *Agaricus Elvensis* from Kingsbury. W. G. Smith sent *Hydnum coralloides* from

Newark. C. Bucknall brought *Cortinarius papulosus* from near Bristol. T. Howse also sent a box of specimens from Guildford. Excepting *Ag. melleus*, the white-spored Agarics were very scarce.

Polyporus intybaceus was found for the first time in Herefordshire. *Geaster fimbriatus* occurred plentifully in Eastnor Park. *Lactarius flexuosus* was again found in Haywood Forest. *Hygrophorus cossus* rather plentifully at Dinmore, but novelties were conspicuously absent, and critical discussion unusually wanting in vigour for lack of material. Some of the sub-genera were not represented by a single species.

Very large specimens of *Agaricus melleus* were measured in Haywood Forest, ten, and ten-and-a-half, inches in diameter of the pileus. Curious malformations of the same ubiquitous species were found at Dinmore.

HERTFORDSHIRE NATURAL HISTORY SOCIETY.—The Cryptogamic Meeting and Fungus Foray in the neighbourhood of St. Albans was held on Saturday afternoon, November 1st. The leaves had been falling briskly for two or three days, and consequently covered many of the few species of Fungi on the ground. Two or three small woods and Gorhambury Park were explored, but only 43 species were recorded, of which 15 had not been recorded for previous Forays. Nearly all the Fungi found were of common species, and these represented by few individuals. The most noteworthy species was *Agaricus* (*Collybia*) *longipes*, Bull. Numerous specimens of *Agaricus* (*Tricholoma*) *personatus*, in excellent condition, were taken from Gorhambury Park, and operated upon afterwards to test their esculent qualities, as also were several individuals of *Ag.* (*Tricholoma*) *nudus*, in both cases with satisfactory results. The Fungi found at the Foray were determined by M. C. Cooke and Worthington G. Smith.

These are the only Forays of which we are enabled to report from personal observation. The general impression in all localities is, undoubtedly, that the number of Fungi seen was far inferior to that of very many previous years. Some say "the worst for twenty years."—*Grevillea.*

KOCH'S "COMMA" BACILLUS.

THE discussions on Koch's "comma," or cholera bacillus show no signs of abatement, at least in Germany, but there are plentiful indications of a decided reaction in Koch's favour after the recent severe attacks upon him. It is pointed out that even Dr. Klein's feat, that of swallowing a number of the microbes, is really no satisfactory refutation of the Berlin microscopist's opinions, inasmuch as the organisms thus imbibed might be "attenuated" specimens, and, moreover, the vigour of the experimentalist's digestive organs at the time might be more than a match for the attacking contingent. Certainly it may be assumed that any earnest seeker after truth whose thirst for knowledge might induce him to swallow a real "cholera mixture" would at least take care to have the ordinary conditions of immunity from enteric disorder on his side. As for the alleged resemblance between the supposed cholera bacillus and other apparently harmless microbes, we have already quoted Koch's own statement that such resemblance is only superficial, and to an experienced observer is not, even so far, perfect; while careful culture shows that the biological and chemical qualities of the former are very different from those of the latter. At the recent general meeting of the Lower Rhine Naturalist and Sanitary Society at Bonn, Dr. Ungar confirmed Koch's statements, and even Dr. Finkler, who, in conjunction with Dr. Prior, lately published an adverse criticism of Koch's report, seems to have admitted that notwithstanding the identity in many respects between the cholera and other similarly shaped bacilli which he and his colleague claim to have established, the poisonous or chemical reactions might be very different. The most practical evidence in favour of Koch's views was given, however, by Professor Binz, who quoted a communication from a Naples physician well known in the scientific world, Professor A. Cantani. Acting upon Koch's discovery that acids are inimical to the true "comma" bacillus, Cantani sought for a free acid mixture which could be administered to cholera patients without injury to them. This he found in a mixture of tannic acid, gum, opium, and water, heated to 38 degrees centigrade, and injected directly into the intestine. Professor Cantani declares that the astonishing results which he obtained "in hundreds and hundreds of cases" during the recent epidemic in Naples have convinced him that the general introduction of this method during the earlier stages of choleraic diarrhœa would reduce the development of the more severe types of the disorder to a minimum hitherto not even hoped for. The

acid mixture does not kill the existing bacilli, but prevents their further development. There seems to be still a prospect, therefore, that the world will have reason to be grateful to the German Government for sending a cholera commission to India, and to Dr. Koch for his discovery of the "comma" bacillus.

OUR BOOK-SHELF.

Our Insect Allies. By Theodore Wood. London: Society for Promoting Christian Knowledge.

This very pleasing little volume which, we trust, will serve for many a Christmas present to those who have just commenced the use of the microscope, is intended to show us that many insects which we have been led to regard from our youth up as injurious to mankind are really helping in the great work of Nature. The author remarks with truth that some few insects are more fortunate than their fellows, and are protected by a superstition which makes it "unlucky" for any one to harm them. This is quite correct, but *why* some should be thus favoured it would be a puzzle to indicate.

There is no doubt if we knew, and knew perfectly, that every insect has its apportioned work set for it in Nature's workshop, and if devastation occurs amongst our crops or in our stores, may not it be due to something we have done, or to something we have omitted? and, in this way, may we not have upset the balance of nature? Little things, so small indeed as to be entirely overlooked by the majority of observers, often effect complete revolutions when allowed to pass undisturbed.

Does not the progress of civilization, as we are prone to call it, often scar the face of this fair earth with such seams as will take centuries to repair? On page 20 our author writes:—"Let us take, for example, the Locust. We can scarcely imagine from our point of view a more terrible enemy than this insect which, appearing in countless myriads, leaves the country over which it passes as bare of vegetation as though it had been scorched by fire."

O man! thou little thinkest how the locust may look at thee, and how thou appearest under his eye.

Not a century ago, the country near to Widnes or Runcorn gap was a beautiful damson growing district, and the gardens of Parr Hall in St. Helens were objects of beauty, but civilization has fallen on the spot, the world must have soda, and alkali, so 'tis said, and

by the consequence of man's action, not a green blade of grass or a healthy tree is to be seen near these spots.

O poor locust! why do they persecute thee for eating for thy daily wants, what man causes to perish from his thirst for gold? But, apart from this reverie, it would be very advantageous to us as a nation if all of us were a little better educated in the matter of insect life, and this little treatise seems to supply a real want. We have a chapter on burying beetles and their kin, the blow-fly and the flesh-fly, dor beetles, scavengers of the waters, "Blight," and its enemies, wood-boring beetles, and the work is illustrated with nearly seventy woodcuts.

We feel sure that nothing will do more to stem the tide of atheism than the publication of small popular handbooks like these, and when the young student has been brought face to face with all the minutiæ of insect life, the details of their structure and their various modes of living, we feel convinced he will never venture to suppose that all these well-ordered details are the result of accident, and then for ever he will believe the old text, "The fool hath said in his heart: there is no God."

NOTES AND QUERIES.

NOTICE.—As this Journal will be suspended for some time, after the issue of the December Number and Index, Subscribers are requested to pay their outstanding Subscriptions at their earliest convenience to Messrs. Brook and Chrystal, 11, Market-street, Manchester.

PRESENTATION.—At the last meeting of the Manchester Microscopical Society, several microscopes were presented to the Society by various members. In all, the gift amounted to over forty pounds sterling.

These microscopes are intended to be used at the ordinary monthly meetings, and to save members the trouble of bringing down their instruments when one is required for the purposes of demonstration.

LIVERPOOL MICROSCOPICAL SOCIETY.—At the December meeting of this Society, a paper was read by Mr. A. T. Smith, Jun., on

the structure of *Alcyonium digitatum*, and in the conversazione which followed, the Hon. Sec., Mr. I. C. Thompson, exhibited a slide of the same showing Zooids and spicules.

NAIAS GRAMINEA.—We have received a brochure on this subject from Mr. Charles Bailey, F.L.S., being a résumé of communications made to the Leeuwenhoek Microscopical Club, and to the Manchester Literary and Philosophical Society. The matter has been reprinted from the *Journal of Botany*, and does Mr. Bailey much credit, from the exhaustive manner in which he has worked at his subject.

OUR SUSPENDED PUBLICATION.—Several correspondents have written to know whether there is any possibility of our resuming publication later on? To this we answer yes! but at present the Editor's time is so occupied that he could not think of carrying on The Microscopical News unaided, for no sooner is one month's number out, than the worry of the next commences. Few people are aware of the vast amount of work required to keep even a small journal like this in motion.

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